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FACTOR ANALYSIS OF MEMORY ABILITY

BY HILDING B. CARLSON

This study is an attempt to analyze memory ability with the use of the Thurstone multiple factor technique.[1] The experimental conditions were arranged in order to test the truth of two very common assumptions: (1) that memory ability can be divided into rote and logical memory, and (2) that rote memory can further be divided into a visual and a vocal factor. The term vocal, as used here, refers to the kinaesthetic and auditory cues that result if an individual pronounces a word, even though inaudibly, inasmuch as there is no adequate means of differentiating them in this circumstance.

That material may be memorized by at least two methods: the rote and the logical, is well known (1). Since the efficiency of memorization may vary with the method used and since individuals may differ in the degree to which they utilize these methods, individual differences in memory ability may be due to differences in the relative effectiveness of the method utilized. In other words, there is the hypothesis that memory ability may be divided into at least two factors: rote and logical. This general assumption of the existence of these two types will be tested.

Even though the material has been presented visually in

[1] From the Psychological Laboratory of the University of Chicago.

The writer wishes to acknowledge his indebtedness to Dr. Frederick H. Wezeman, President, Chicago Christian High School and Junior College, where the majority of the students who participated in this experiment were obtained.

this experiment, since this is the most feasible manner, the mode of presentation does not necessarily determine the modality which will be adopted by a subject in memorizing that material (3). The efficiency of memorization may vary with the sense modality utilized, even though the group methods previously used to determine this have given inconsistent results. Since individuals may differ in the degree to which they use various modes, individual differences in memory ability may be due to differences in the relative effectiveness of the sense modality used. There is the hypothesis, then, that memory ability may be divided into visual and vocal memory. This assumption will also be tested.

As stated, previous experiments comparing memory efficiency of the different senses have found no consistent differences between groups. There is the possibility that vision is superior for some individuals, and audition for others, and that the two cancel each other for the group as a whole. The group method merely discovers what is true for the group as a whole, and gives no way of discovering the possibility of intragroup variability in respect to the factor studied. The multiple factor method, developed in recent years, however, does this sort of thing. It is a method of isolating the various abilities in respect to which individuals may be classed into types of the general sort. With this new technique, it may be possible to demonstrate that memory efficiency does vary with the sense modality utilized.

One way of attempting to elicit the postulated logical, visual, and vocal factors would be to vary the character of the material, and this has been done here. Various kinds of material might have been utilized, but since words are much better, especially for presentation, they have been used. Their use necessitated employing the recognition method of testing memory. Recall or relearning might have been employed, if words and nonsense syllables were used, but this would have necessitated lists of nonsense syllables of such character that some would have pronounced vocal characteristics and others visual, and there is no available criterion for selecting nonsense syllables having these characteristics.

With the employment of the recognition method, specific distractors had to be used in order to obtain a differentiation. Accordingly, it was necessary to make up lists systematically varying the distractors in visual appearance, vocality, and meaning, and in their combinations, in order that each mode be separately favored. In order to do this, it was necessary to select groups of words of such character that other words could be found that have particular relationships to the first words. One group of words was selected because for each word a synonym could be found; another group of words was chosen because for each word a homonym could be found. In like manner, other groups of words were selected as indicated in Table 1. This table also gives an example of each group.

TABLE 1

RELATION BETWEEN THE WORDS OF A PAIR IN THE EXPOSURE AND RECOGNITION LISTS

Group	Number Words in Each Recognition List	Words Alike in Respect to:	Words Different in Respect to:	Example
A	56	.....	Visual appearance Vocality Meaning	cruel diagonal
B	50	Vocality	Visual appearance Meaning	no know
C	48	Meaning	Visual appearance Vocality	rely depend
D	24	Visual appearance	Vocality Meaning	ob'ject ob-ject'
E	44	Vocality Meaning	Visual appearance	through thru
F	26	Visual appearance Meaning	Vocality	con'tem-plate con-tem'plate
G	66	Visual appearance Vocality	Meaning	bark (of dog) bark (of tree)

Inasmuch as the words were presented visually, and the words of a pair in Groups D and F are alike in spelling but

different in vocality, it was necessary to designate their pronunciation in some way in order to differentiate them. It was entirely arbitrary as to how this was to be done. Accordingly, the simplest method was chosen, namely, by spelling the words in syllables and indicating the place of the spoken accent by the usual character. This manner of designation excluded words which have two different pronunciations but which can be differentiated only by the use of phonetic symbols. It is probable that the majority of subjects are not familiar enough with these symbols to use them with facility. In addition, their use would have resulted in additional visual cues.

Ideally, in Group G both words of a pair should have the same visual appearance and vocality but should differ in meaning. Because of the impossibility of designating a specific meaning for these words without the use of context, it was necessary to indicate the meaning intended. This was done by placing the specific meaning in parentheses, as 'bark (of dog)' for one and 'bark (of tree)' for the other. This group does not adequately fulfil the logical experimental requirements inasmuch as subjects could react to the words in the parentheses, as '(of dog)' and '(of tree)' and disregard the word 'bark' entirely. To minimize this possibility, the words in parentheses were made less conspicuous by using smaller type.

One of each pair of words was to be included in an exposure list. Both words of the pair were to be included in a recognition list, the word not used in the exposure list serving as a distractor for the other. The relation between the associated words in a recognition list was to be constant, so that the use of a particular mode or combination of modes is favored. It is not essential that the lists be equal in length because we are concerned only with the relative scores made within the group. However, it was important to make the lists difficult enough to insure differentiation among the subjects. At the same time, it was undesirable to make them so difficult that the subjects would become discouraged and lose interest in the experiment. After preliminary trials with a small group of subjects, an attempt was made to make the lists of such

length that the average score would approximate 75 percent correct recognition. The number of words used in the recognition lists for each group in the experiment proper is included in Table 1.

In this experiment, where the material, the procedure, and the method of analysis have been definitely restricted, a single list of each group of words would not be sufficient for the solution of the problem. There were three factors determining the number of lists: enough lists to insure a reasonable degree of reliability; avoidance of making the experiment too long; and the number of pairs of words in each group that can be procured. Five lists were considered sufficient to satisfy the first and yet comply with the second. Due to the third, however, only four lists could be made for Group D.

In order to make up these lists with all the requirements and limitations imposed, it was necessary to disregard certain differences among the words, such as syllabic length, parts of speech, etc. No word is used twice. All the words were checked against Webster's Unabridged Dictionary (7). An attempt was made to select from all the available pairs of words those which were considered to be most familiar to the subjects.

The order of arrangement of the words in both lists is by chance except that one member of the pair of words does not immediately follow the other in the recognition list. The words in the recognition lists were numbered. The lists were labeled A1, A2, A3, A4, A5, B1, B2, etc.

To make it possible to give the experiment to a group of subjects at one time and to control the exposure time for each word, the words were serially exposed on a screen by means of a film and a projection machine. Light letters on a dark background were used instead of the more usual black letters on a light background. This was done to save time and expense but it should in no way influence the results inasmuch as it is a constant factor for all subjects and for all words.

The exposure list was presented to the subjects. They were told that a series of words would be shown on the screen.

They were instructed to memorize the words so as to be able to recognize them when presented as part of a longer list. No instructions as to the mode of memorizing were given. The list was presented but once. Each word was exposed for two seconds.

The recognition list was shown immediately after the exposure list. The subjects were told that all the words in the first (exposure) list were in the second (recognition) list, interspersed with other words. Nothing was said concerning the nature of the distractors. No mention was made that the recognition list was twice as long as the exposure list. The subjects were asked to indicate on a prepared data sheet which words were in the exposure list and which were not. They were told to guess whenever in doubt. Thus a response was made to every word in the recognition list. This list was also exposed but once. The exposure time for each word was three seconds, one second longer than in the exposure list, to permit the subject time to mark a check on the data sheet.

With this manner of presentation and of recording the responses, very close attention was required of the subjects. If a subject was diverted while a word in the recognition list was shown, all succeeding responses would be misplaced on the data sheet. In an attempt to avoid this source of error, the assistant, operating the projection machine, called out the number of each word in the recognition list as it was presented. This may have functioned as a distractor that may have differentially influenced the subjects, but since it was done for all the recognition lists, it should not influence the results.

As the majority of the subjects were not familiar with the experimental set-up, five preliminary practice lists were given in order to prevent as much as possible the variability in the scores that might result from this lack of familiarity with the testing procedure. These practice lists were similar to the lists in Group A in which the confusion words are unrelated to the words in the exposure list. No use was made of these data.

The lists in the experiment proper were presented to the

subjects in the following order: A1, B1, C1, D1, E1, F1, G1, A2, B2, etc., until all the lists were given.

The complete battery required three hours of testing time. In order to offset as much as possible the effects of fatigue, boredom, etc., no test period was longer than one hour.

To increase the reliability of the tests as indices of the ability of the subjects to memorize and recognize words, the experiment was repeated after an interval of two to three months. The average score of the two presentations was then taken as an approximation of the true score.

The number of subjects present at a test period varied from 6 to 35, averaging about 20. A total of 202 subjects, 121 girls and 81 boys, voluntarily completed the experiment. The records for 55 others were discarded because they were incomplete or because the subject was observed to give poor co-operation. The subjects were 15 years of age or over and had completed at least the freshman year of high school.

There are two types of errors possible: errors of commission and errors of omission. The first occurs when a subject indicates a distractor word in the recognition list as having been present in the exposure list; the second, when a subject indicates an exposure list word in the recognition list as not having been present in the exposure list. In this experiment no attempt was made to distinguish between these errors, a subject's score being the total number of correct discriminations between the exposure list words and the confusion words. Stencils were used to facilitate the scoring.

The frequency distributions of the average individual scores of each of the lists for the two presentations are more or less bell-shaped and fairly symmetrical. At the present time it seems reasonable to assume that the ability to memorize this material is normally distributed in the experimental population. This assumption is identical with that involved in the tetrachoric correlation coefficients. While the probable error of a tetrachoric correlation is larger than the probable error of a Pearson product-moment coefficient, the slight increase in accuracy in the use of the latter does not justify the much greater amount of work entailed in its cal-

TABLE 2

Intercorrelations of the Variables

Variable	A2	A3	A4	A5	B1	B2	B3	B4	B5	C1	C2	C3	C4	C5	D1	D2	D3	D4	E1	E2	E3	E4	E5	F1	F2	F3	F4	F5	G1	G2	G3	G4	G5
A1	.56	.62	.60	.66	.71	.67	.58	.66	.65	.72	.70	.40	.59	.52	.31	.43	.44	.51	.69	.70	.62	.66	.54	.38	.40	.40	.48	.49	.51	.56	.56	.68	.55
A2	..	.68	.65	.74	.76	.81	.68	.64	.71	.76	.62	.62	.67	.70	.37	.41	.44	.51	.63	.60	.51	.70	.54	.40	.53	.36	.48	.51	.58	.57	.69	.69	.56
A3	..	..	.68	.80	.74	.67	.69	.64	.75	.61	.60	.59	.66	.65	.32	.48	.66	.58	.62	.57	.48	.74	.57	.57	.54	.51	.47	.52	.63	.66	.66	.68	.60
A4	..	..	..	.72	.58	.56	.59	.72	.61	.49	.59	.55	.72	.75	.27	.39	.35	.42	.53	.53	.44	.69	.42	.34	.29	.38	.42	.34	.64	.55	.57	.60	.61
A5	..	..	..	..	.70	.80	.80	.77	.81	.73	.69	.63	.79	.80	.32	.38	.48	.52	.59	.58	.64	.79	.62	.41	.42	.40	.52	.47	.51	.58	.59	.72	.66
B1	..	..	..	..	..	.70	.79	.72	.72	.81	.63	.57	.67	.70	.42	.49	.47	.53	.65	.56	.65	.69	.59	.51	.52	.50	.59	.53	.62	.54	.57	.71	.59
B2	..	..	..	..	..	..	.85	.77	.80	.73	.73	.63	.68	.65	.36	.50	.59	.62	.67	.54	.59	.68	.58	.50	.48	.37	.52	.49	.51	.68	.53	.70	.58
B3	..	..	..	..	..	..	..	.74	.78	.63	.71	.68	.68	.76	.38	.56	.56	.55	.57	.52	.59	.63	.57	.52	.48	.43	.55	.55	.48	.59	.53	.63	.62
B4	..	..	..	..	..	..	..	..	.75	.71	.66	.71	.74	.72	.37	.50	.47	.51	.56	.59	.65	.65	.59	.48	.41	.47	.45	.51	.52	.65	.62	.69	.68
B5	..	..	..	..	..	..	..	..	..	.61	.65	.79	.70	.74	.47	.54	.59	.65	.62	.65	.68	.66	.71	.55	.54	.43	.44	.55	.48	.63	.66	.77	.67
C1	..	..	..	..	..	..	..	..	..	..	.66	.62	.62	.63	.30	.44	.47	.43	.68	.61	.55	.60	.51	.43	.38	.38	.43	.40	.52	.52	.47	.60	.61
C2	..	..	..	..	..	..	..	..	..	..	..	.59	.66	.67	.32	.51	.48	.50	.65	.67	.68	.61	.55	.50	.48	.30	.60	.52	.68	.56	.54	.63	.71
C3	..	..	..	..	..	..	..	..	..	..	..	..	.67	.70	.43	.47	.55	.56	.43	.51	.50	.55	.48	.54	.44	.47	.34	.53	.38	.50	.62	.60	.59
C4	..	..	..	..	..	..	..	..	..	..	..	..	..	.68	.27	.44	.41	.54	.59	.48	.52	.69	.54	.40	.35	.29	.43	.40	.50	.57	.65	.60	.54
C5	..	..	..	..	..	..	..	..	..	..	..	..	..	..	.32	.43	.40	.57	.52	.62	.54	.75	.62	.50	.49	.34	.52	.50	.61	.56	.66	.68	.62
D1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	.40	.48	.41	.29	.32	.40	.37	.32	.38	.51	.54	.44	.41	.24	.26	.27	.37	.41
D2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	.65	.54	.54	.59	.50	.43	.48	.56	.67	.55	.66	.56	.41	.41	.38	.47	.37
D3	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	.66	.40	.40	.32	.50	.56	.66	.60	.47	.66	.54	.35	.52	.50	.44	.37
D4	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	.47	.50	.40	.48	.50	.58	.51	.51	.62	.55	.45	.51	.54	.48	.50
E1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	.71	.68	.75	.65	.55	.54	.21	.41	.44	.72	.66	.45	.68	.65
E2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	.72	.61	.67	.44	.51	.37	.52	.41	.61	.56	.63	.66	.62
E3	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	.71	.81	.45	.52	.41	.45	.48	.60	.60	.57	.65	.61
E4	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	.68	.51	.47	.41	.54	.48	.71	.71	.57	.71	.59
E5	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	.52	.54	.42	.47	.55	.48	.54	.59	.51	.62
F1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	.68	.51	.69	.65	.43	.45	.43	.51	.44
F2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	.57	.79	.68	.50	.55	.52	.57	.48
F3	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	.56	.70	.22	.29	.50	.35	.45
F4	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	.71	.48	.48	.48	.56	.52
F5	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	.43	.37	.56	.48	.52
G1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	.80	.65	.80	.70
G2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	.69	.80	.72
G3	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	.78	.70
G4	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	.72
G5	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..

culation. Accordingly, tetrachoric correlation coefficients were used in this study.

They were determined for all pairs of scores by means of computing diagrams (**6**). Each variate was divided at or near the median to avoid the increased margin of error when the division is made at either tail of the distribution. The correlation coefficients were read to two decimal places. They are recorded in Table 2. The mean correlation is .59, and its probable error, computed by the formula given by Kelley (**2**, p. 258), is .055.

The resulting correlational matrix of order 34 × 34 was factored to eight dimensions by the centroid method, following the procedure outlined in Appendix I of *The Vectors of Mind* (**4**).

After the eighth factor had been extracted, the residuals had a mean absolute value of .029 with a standard deviation of .036. Inasmuch as the largest contribution of the eighth factor to any correlation was only .06, and any further factor would probably contribute less, no further analysis was made.

These factors, described in terms of the arbitrary orthogonal axes of the centroid analysis, are not psychologically meaningful. However, a meaningful solution is obtained when the factorial matrix has been rotated so that the number of zero entries is maximized and so that the non-vanishing entries are positive. In this study the graphical method (**4**) was used in an attempt to transform the arbitrary orthogonal matrix of the centroid analysis into orthogonal simple structure.

Since the resulting transformed factorial matrix (Table 3) is orthogonal, the factors are independent of one another. It will be noticed that there are no zero entries in the first column of this matrix, indicating that the orthogonal structure found is not overdetermined and therefore not unique. However, the orthogonal structure found does not seem unreasonable. All of the tests in the battery are similar in that they involve the visual perception, memory, and visual recognition of words; all were appraised in the same manner, etc. As is to be expected, all of the correlations are positive

and fairly high. In fact, if the transformed matrix did not show a general factor in this experimental set-up, we might well question it.

TABLE 3

TRANSFORMED ORTHOGONAL MATRIX

Variable	I	II	III	IV	V	VI	VII	VIII
A1	.72	−.01	.13	−.05	.21	.00	.32	−.07
A2	.77	−.04	−.06	.11	.30	.22	.00	.01
A3	.65	.07	.03	.23	.36	.40	.20	.06
A4	.67	−.15	−.15	.10	.27	.05	.36	.24
A5	.82	−.15	−.02	.04	.25	.29	.04	.08
B1	.81	.06	.05	−.01	.40	.12	.00	−.03
B2	.81	−.02	−.01	.00	.07	.43	.03	−.07
B3	.82	.02	−.02	−.06	.05	.37	−.05	.07
B4	.82	−.09	.04	−.06	.12	.16	.13	.23
B5	.82	−.01	.12	.04	.05	.36	−.04	.22
C1	.79	−.09	.03	−.12	.25	.14	.07	−.12
C2	.84	.03	−.04	.05	−.10	.05	.23	−.05
C3	.71	.03	−.05	−.07	−.02	.31	−.03	.38
C4	.74	−.15	−.04	−.02	.14	.23	.20	.13
C5	.79	−.03	−.07	.12	.13	.17	.06	.19
D1	.37	.35	.13	−.05	.21	.13	−.02	.24
D2	.57	.46	.13	−.17	−.04	.16	.25	.00
D3	.43	.44	.05	.05	.06	.57	.26	.06
D4	.56	.32	−.05	.01	.00	.35	.26	.18
E1	.72	.01	.30	.15	.12	.09	.29	−.33
E2	.75	.07	.28	.09	−.05	−.06	.28	.03
E3	.75	.02	.59	.08	−.01	−.05	.03	.05
E4	.71	.00	.18	.21	.34	.20	.23	−.02
E5	.64	.10	.53	.11	−.01	.26	.06	.11
F1	.54	.51	.07	.08	.02	.28	.09	−.04
F2	.58	.63	.13	.22	.04	.12	−.04	−.05
F3	.41	.53	.13	−.04	.34	.03	.05	.40
F4	.61	.65	−.04	.10	.02	.07	.17	−.05
F5	.61	.57	.04	.08	.09	.04	−.06	.17
G1	.64	.01	.07	.54	.10	−.02	.37	−.05
G2	.63	−.04	.15	.50	.00	.27	.28	.03
G3	.68	.07	.02	.36	.04	.09	.10	.36
G4	.83	.01	−.02	.43	.09	.02	.06	.01
G5	.72	.03	.10	.33	.08	.02	.15	.25

The interpretations of the psychological significance of all of the factors in Table 3 cannot be made with certainty. There is, of course, no necessity to name and define them, but unless it is possible to do so their isolation is not of much psychological significance. Therefore tentative interpreta-

tions will be made, if possible. These are not to be considered as final definitions of the factors, but rather as in the nature of further hypotheses requiring verification.

Factor I has large and significant loadings on every test. It is, therefore, a general factor for these materials, and it is possible that it may have a wider degree of generality than is indicated by these tests. With the present experimental set-up there is no way to determine sharply the limits of this factor. For the present, it can only be defined in terms of the general characteristics of these materials and operations. Accordingly, it is termed a general recognition memory factor for words of these characteristics.

In a recent comprehensive study Thurstone (**5**) includes in a battery a recognition test of individual words with specially selected lists of distractors. He reports that a factor M (memory) and a factor W1 (word facility) each have appreciable loadings on this test. Inasmuch as there were no tests included in the present battery which did not involve the use of words and memory, it is impossible to isolate similar factors in this study. If the tests in the present battery had been included in Thurstone's study, it is possible that the general factor found in this study would have been split up into his memory and word facility factors.

The simplest explanation for Factors II, III, and IV is that specific visual cues are involved in each. However, there are alternative explanations for Factors II and IV, and these will be discussed individually.

An alternative explanation for Factor II is that it may be a restricted vocality factor. This factor has high loadings for all the tests of Groups F and D and for none of the others. In the tests of Group F the exposure word and its distractor differ only in vocality; in the tests in Group D, they differ in vocality and meaning. In both groups the related words differ in vocality, and there is, therefore, some justification for naming it vocality. It would not be surprising to find vocality is a function involved in the visual recognition memory of words in this experiment for a number of the subjects were observed to move their lips, silently enunciating the

words in their attempt to memorize them. But since vocality might also have been used to advantage in tests in Groups A and C a vocality factor might be expected to have loadings in the tests in these two groups if individuals were consistent in enunciating all the words. Factor II has zero or very small loadings on these tests, and therefore defining this factor as vocality might well be questioned.

The pairs of words in Groups A and C differ in vocality. It does not follow, however, that because certain cues are present they will necessarily be used. The subjects will undoubtedly have a tendency to use whichever method makes it easiest for them to differentiate the words of a pair. Since the words were presented visually, it is probable that the majority of the subjects attempted to use visual cues. By means of visual cues, it is possible to distinguish fairly readily between the words of a pair in the lists in Groups A, B, C, E, and G. On the other hand, the related words in Groups F and D are alike in spelling, differing (from the standpoint of vision) only in syllabication and in position of the accent mark, and cannot be distinguished as readily on the basis of vision. It would appear, then, that vocal cues were used to advantage in this experiment only when the visual cues were not adequate.

Factor III has large loadings on tests E3 and E5; the next highest loadings are on E1, E2, and E4, in the order named. In these lists the only difference between the related words is in spelling. Accordingly, the subjects could not distinguish between them if they used vocality or meaning, but they could if they used visual cues. Visual cues is a class name for visual distinctions, and there may be a number of these visual cue factors. Since it would be of interest if the visual cues involved in this factor could be specifically determined, the material comprising these tests was further investigated. This revealed that of these tests E3 has the largest number of pairs of words with the same letters but with an inversion of the letters 'e' and 'r,' whereas E2 and E4 have the smallest number, and E3 has the smallest average number of letters not common to the words of a pair,

whereas E2 has the largest average. This would seem to indicate an ability to utilize the internal structure of the words, or the arrangement of the letters, as contrasted with grosser visual cues and non-visual cues. While this is not proof that these cues are the only ones involved in this visual factor, it is suggestive that they are at least involved.

The fourth factor has large and significant loadings in all the tests in Group G and in no others. As has been pointed out, this factor may indicate an ability to use a specific visual cue. An alternative explanation, however, is that it indicates an ability to utilize a specific meaning in the memorization and recognition of a word. Since meaning could also have been used to advantage in the tests in Groups A, B, and D, why did this factor not have significant loadings in these tests? There are at least two possible explanations, the correctness of which can only be determined by further experimentation. In the first place, the subjects may have found it convenient to use meaning when it was specified, as was done in the tests in Group G, but found it easier to use another method when the meaning was not given explicitly. Because meaningful cues are present does not mean necessarily that they are or must be used. The second explanation, to which the writer would lean, is that a different function is involved when a subject passively uses meaning when it is specified for a word than when a subject actively recalls and uses the meaning of a word even though it is not specified, as must be done if meaning is to be used in the tests in Groups A, B, and D. It is reasonable to assume that there are many more individuals who can appreciate the meaning of an object or event when it has been pointed out to them than there are individuals who have originality enough to determine of their own efforts the meaning of the object or event. The two meaning abilities may be uncorrelated in which case another factor would be expected to be found that may be interpreted as active meaning. Such a factor may be VI.

The remaining factors, except Factor VI, are less significant than the preceding ones, and they probably cannot

be differentiated from the variable errors and certainly cannot be named with confidence. It is probable that the data did not justify the number of factors extracted from it. In the preceding factors the interpretations were made as though the only source of incorrect response is to be found in the relation between the paired words. It is obvious, of course, that there are other sources of error as well. If similar sources of error are present in two or more tests, but not in all the tests, the end result may be a factor whose nature is not readily discernible from the tests in which the factor has loadings. There were occasional disturbances in the corridor due to basket ball games, etc., which could not be controlled. The recognition tests used in this experiment are particularly sensitive to these external distractors because of the manner of presentation and of recording the responses. Since the number of subjects present in a group at one time varied from six to thirty-five, it is probable that the subjects in one or more of these small groups were influenced by external distractors on certain tests and not on others. The end result may be a factor or factors whose nature would not be evident unless a comparison could be made with the tests being given at the time such external distractors occurred, but this cannot be done since no record was kept of these distractors. With these and other considerations in mind it would be surprising if all the factors could be satisfactorily interpreted. As a matter of fact, no interpretation making psychological sense could be made for Factors V, VII, and VIII.

As suggested in the discussion of the fourth factor, the sixth factor may be interpreted as 'active meaning.' This factor has significant loadings on the last two tests in Group D; on the second, third, and last of Group B; and the third test in Groups A and C. This factor may indicate an ability to learn to utilize the meaning of a word in its memorization, even though that meaning is not explicitly given, since in all the tests in Groups A, B, and D meaning could be utilized to discriminate between the exposure word and its related word in the recognition list, and since this factor does not have significant loadings in the first two tests of Groups A

and D, and the first test in B. This learning would imply a more or less active attempt on the part of some of the subjects to use meaning for the memorization of a word under the conditions of the experiment. It is the opinion of the writer, therefore, that there is some justification for interpreting this factor as 'active meaning' or some comparable term. This interpretation, however, can only be tentative subject to verification as there are a number of objections to it. The presence of a loading on test C_3 would require explanation, as would also the absence of loadings on B_4, A_4, and A_5, and the fact that D_3 is greater than D_4. In C_3, even though the related words are synonyms, there nevertheless is some shade of difference in their meaning, and it is possible that a number of words in the exposure list have other more familiar associations for some of the subjects than the synonymous distractor word used, in which case meaning might be used to advantage. The explanation of the other discrepancies is not so apparent.

Conclusions

The assumptions were made that memory may be divided into rote and logical memory and that rote memory may be further divided into visual and vocal memory. Since the interpretations of the factors found are equivocal the truth of these hypotheses is not definitely established.

The simplest interpretation of Factors II, III, and IV is that they are three different visual factors. If this is correct, the experiment demonstrates that there are a number of visual memory factors instead of the single visual factor postulated.

An alternative interpretation for Factor II is that it is vocality. If this is correct, it would appear that the subjects resorted to vocality under the conditions of the experiment only when vocal cues were emphasized and visual cues were minimized.

An alternative interpretation for Factor IV is that it is a logical factor in which meaning is explicitly given. If further research verifies this, and the tentative interpretation of Factor VI as being a logical factor involving learning to use

the meaning of a word even though the meaning is not explicitly given, it would appear that logical memory is itself not unique, but rather complex.

If the alternative interpretations of Factors II and IV are correct and if Factor III is a visual factor, the experiment has differentiated between a rote and a logical factor, and the rote factor is further subdivided into a visual and a vocal factor as postulated.

From the statistical analysis, it appears that a general factor, common to all the tests in this battery, is by far the most important single factor involved in the memory of words. Further research will be necessary to demonstrate whether this factor has a greater degree of generality than is indicated by the tests included in this battery.

Three factors appear whose psychological significance is not apparent.

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References

1. Carr, H. A., *Psychology.* New York: Longmans, Green, 1926.
2. Kelley, T. L., *Statistical Method.* New York: Macmillan, 1928.
3. O'Brien, F. J., A qualitative investigation of the effect of mode of presentation upon the process of learning, *Amer. J. Psychol.*, 1921, **32**, 249–283.
4. Thurstone, L. L., *The Vectors of Mind.* Chicago: Univ. of Chicago Press, 1935.
5. Thurstone, L. L., The factorial isolation of primary abilities, *Psychometrika*, 1936, **1**, 175–182.
6. Thurstone, L. L., Chesire, L., and Saffir, M., *Computing Diagrams for the Tetrachoric Correlation Coefficient.* Chicago: Univ. of Chicago Bookstore, 1933.
7. *Webster's Unabridged Dictionary.* Edited by W. T. Haines. Springfield, Mass.: Merriam Co., 1930.

AN EXPERIMENTAL DEMONSTRATION OF REGRESSION IN THE RAT

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I. Introduction

The terms 'fixation' and 'regression' have long been used as applying to certain abnormal aspects of human behavior, and more recently there have been various types of study including them in the terminology describing animal behavior. Hamilton and Krechevsky (**8**) describe an experiment with rats in which shock produced a complete fixation upon one mode of behavior, and further found evidence to support the view that this fixation, at least in some cases, was in the direction of an earlier learned pattern. Everall (**2**) has recently added data to strengthen this position, while Martin (**10**) reports failure to demonstrate regression. Studies of the more general phenomenon of fixation include work by Gilhousen (**4**), who found that rats fixated upon a learned long pathway to food even when a shorter pathway was open to them; by Elliott (**1**), who suggests that strong motivation may be a fixating agent; and by Hamilton and Ellis (**6**, **7**) showing fixation to be a function of cortical destruction. Krechevsky and Honzik (**9**) found fixation, or lack of docility in behavior, resulting from excess training on one pathway of a maze. An experiment by the present writer (**12**) showed that fixation upon a single response could be produced by shock in animals trained equally in two modes of response (*i.e.* simple alternation).

Tolman and Krechevsky (**14**) suggest that the study of fixation and regression is a study of the laws of unlearning, defining this unlearning as 'the reversion back from a more specific hypothesis to a more general means-end-readiness.' In the sense used here, the reversion is caused by a disrupting stimulus situation, and the behavior pattern emerging is the

one most dominant in the disturbed organism. Tolman and Krechevsky also indicate that native factors may be significant in making one pattern or hypothesis dominant over the others. This is a thesis which we shall uphold. Further, the apparent necessity for a disrupting factor in initiating regression suggests that regression is caused by an emotional condition. We shall refer to this point later.

For the purposes of this experiment, the definition of regression given by Hamilton and Krechevsky (**8**) is probably the best:

> "Regression may be defined as a reversion to an earlier, well-established mode of behavior, and persistence in that mode despite the relative inefficiency of that behavior in solving the problem confronting the organism. Further, regression has been assumed to arise when the individual is placed in a strong emotional situation."

Moreover, we wish to add to this definition the possibilities that regression is to a form of behavior lower ontogenetically (Wells (**15**) suggests this in his discussion of the genetic aspect of regression), that it is not docile, and that the adjustment may not be adaptive by any objective standard.

The experimenter has attempted, first by training the animals in two patterns of response, one following a genetic 'bias' of the animal and the other the opposite of this bias, and then by subjecting them to various conditions calculated to create an emotional disturbance, to isolate the behavior phenomenon regression, and to discover something of its nature and the conditions under which it occurs.

II. Apparatus, Animals, and Procedure

A walled maze, shaped in the form of a cross, was the apparatus used here. (See Fig. 1.) The two routes to food, *ABCEF* and *ABDEF*, are spatially of equal length, but by detaining the animals for various periods of time in the chamber set off by gates G_1 and G_2, the paths may be made temporally different. These gates were so arranged that the experimenter (at *X*) could raise or lower them at will. An electric grid covered the floor of the alley *AB* at all times. Other pieces of apparatus, used at different points during the procedure, will be described later.

Five male rats, 2 to 3 months of age and previously untrained, were used in this experiment.

Preliminary training.—On days 1 and 2 the animals were placed in the apparatus, with all gates open, and were allowed to become thoroughly familiar with the situation. On the third day, each animal found food at *F* after the period of exploration. Training began on the fourth day.

For ten days, each animal was allowed to run through the apparatus ten trials

a day, with food at *F* after each run, and with both gates open. At the conclusion of the ten-run series, the rats were fed their day's rations. This schedule of ten trials followed by feeding was maintained throughout the experiment.

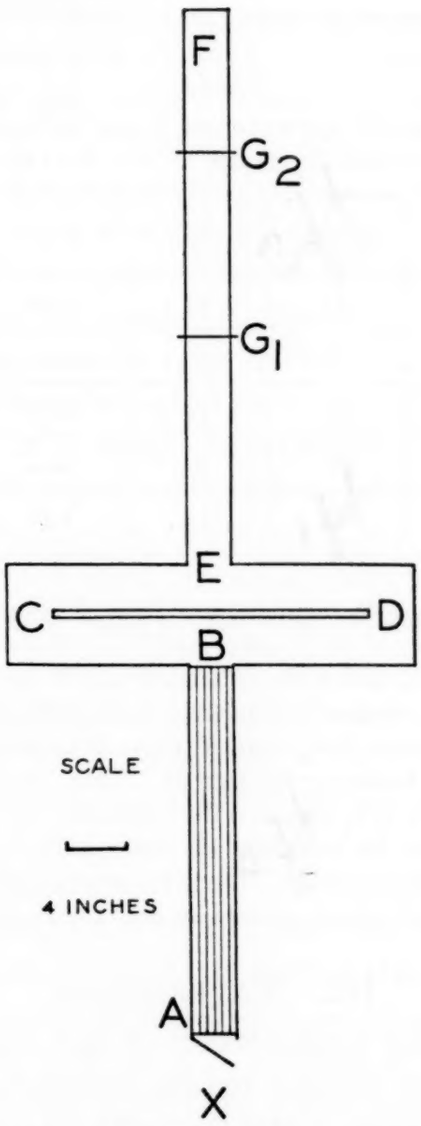

FIG. 1. Diagram showing floor plan of apparatus used.

Since during these first trials both the left and right paths were equally accessible to food, we may assume that a predominance of one choice over the other is conditioned by any natural position or turning preference the animal may have. The differences between the percentages of left and right choices made by the animals were found to be statistically significant in every case. The formula used was Yule's formula for finding the critical ratio, C. R. = $D/\sigma D$.[1] Table 1 shows the critical ratios obtained by this method.

As the result of experimentation by Yoshioka (**16**, **17**) and Gengerelli (**3**), position preferences may be regarded as unlearned modes of response. Since the purpose of

[1] Yule, G. V. *An Introduction to the Theory of Statistics*, 1924, pp. 269. Formula 6.

this experiment is to find whether an animal reverts from one reaction pattern to an earlier (possibly an inherited) one under conditions of stress, the advantage of having the earlier pattern based on an unlearned tendency is apparent. Thus the natural tendency shown by the animal was made his pattern I (see Table 1). It was strengthened by making it the temporally shorter path to food.

TABLE 1

SHOWING THE C. R. OF THE D. BETWEEN THE R AND L CHOICES, AND THE RESULTING FIRST AND SECOND PATTERNS OF THE DIFFERENT ANIMALS

Rat No.	No. L Choices	No. R Choices	D.	C. R.	Pat. I	Pat. II
1	71	29	42	6.58	L	R
2	83	17	66	12.21	L	R
3	87	13	74	15.57	L	R
4	65	35	30	4.44	L	R
5	39	61	22	3.18	R	L

Experimental training.—Each animal was given ten runs a day with a five second wait in the delay chamber if the 'correct path' (*i.e.*, the path preferred in the preliminary trials) was taken, and a ninety second wait if the opposite, or wrong path was selected. Training continued until the correct path was chosen for 90 percent of the runs on four successive days, or until 36 out of 40 consecutive trials were in the direction of the natural tendency (pattern I). Then the rats were trained in the pathway opposite to that first chosen. We shall refer to this as pattern II. The reversal was accomplished by switching the delay periods, so that the long delay followed a choice in the first pattern. The same criterion used for pattern I was also used here, *i.e.*, 90 percent correct choices on four successive days. Critical experiments were then begun.

III. EXPERIMENTS

Before beginning a discussion of the experiments, we believe it is necessary for the reader to recall that the animals now had two behavior patterns, with the evidence in favor of the view that one (pattern I) was based on an unlearned tendency, and the other (pattern II) was artificially set up. At any rate, the two had been strengthened by training to the same level of efficiency. After the 'natural' pattern had been learned, it was discarded for the second pattern, which now became the optimal method of obtaining food. The purpose of the experiments now becomes apparent—to see how the balance between these two patterns is affected (*A*) by electric shocks within and (*B*) by electric shocks outside

of the apparatus; (*C*) by the use of a loud sound; (*D* and *E*) by the injection of drugs; and (*F* and *G*) by various degrees of training on the two patterns directly before the shock stimulus is introduced.

No control group was used in this study, because the experimenter feels it may be safely assumed that, if no changes had been made in the situation, the animals would have continued to run in the optimal path (*i.e.*, the path best from the standpoint of obtaining the food reward in the most direct way) for an indefinite period of time, regardless of whether that path followed their natural tendency.

Directly subsequent to each of the experiments discussed below (with the exception of experiment *F*), the animals were retrained in pattern II until the criterion discussed above was again reached. Even in cases where the test situation had no discernible effect, this precaution was used so that the animals entered each new situation with the same training. Thus at the outset of each experiment (with the exception of *G*) pattern II was objectively dominant. Critical experiments were taken up in the following manner:

A. As soon as patterns I and II had been learned, each animal was run in the apparatus just as before, except that an electric shock [2] was given to the rat as he ran the path *AB*. This constitutes test 1. With the exception of this shock just preceding each of the ten choices on the test day, procedure remained the same as during the pattern II training series—the rats were delayed five seconds if they selected the correct path, and ninety seconds if they selected the wrong path. On the following days, the animals were retrained in pattern II (no shock given) until they again reached the criterion previously used, when a second test series, like the first in every detail, was inserted.

From a consideration of Table 2, we can see that, in the case of four of the rats, the shock had a definitely precipitating effect on their behavior, and that this precipitation was in the direction of pattern I. These results are quite in line

[2] 500 volts passing through 3,000,000 ohms resistance, or a current of about .19 milliampere.

with what we would expect if regression took place, a reversion, under conditions of stress, from a newer, more recent mode of behavior to an earlier pattern.

TABLE 2

SHOWING RUNS MADE ON TESTS 1 AND 2 AND ON THE DAYS JUST PRECEDING THE TEST DAYS. EXPERIMENT *A*

Rat No.		1	2	3	4	5
Choices on day preceding test 1	Pattern I.....	0	0	1	1	0
	Pattern II.....	10	10	9	9	10
Choices on test day 1	Pattern I.....	2	10	10	10	10
	Pattern II.....	8	0	0	0	0
Choices on day preceding test 2	Pattern I.....	0	0	1	1	0
	Pattern II.....	10	10	9	9	10
Choices on test day 2	Pattern I.....	2	10	10	9	10
	Pattern II.....	8	0	0	1	0

But the results might also be considered quite adequately under the conditioning concept of disinhibition. According to this view, the pattern first learned was a trace conditioned reflex, pattern I being preferred to pattern II because of the stronger inhibition of delay resulting from choices in pattern II. By the same means, pattern II took the place of pattern I, simply because the experimenter caused the longer delay (ninety seconds) to follow turns in that direction (pattern I) and the shorter delay (five seconds) to follow turns in pattern II. The disinhibition occurring here would be the release (by extraneous stimulation) of the inhibition of delay built up against pattern I. This reasoning agrees with Pavlov's concepts of trace reflexes, inhibition of delay, and disinhibition (**11**). A similar description of disinhibition is given by Skinner (**13**), except that the inhibition released is not inhibition of delay, but another form, extinction. On the basis of results in this experiment we cannot refute the conditioning explanation, but an experiment to be discussed later leads us to doubt its adequacy in this study.

Before going on to other experimentation, we must consider briefly the case of rat no. 1. As Table 2 shows, this animal did not revert to pattern I, as did the other animals, but rather held, on 8 of his 10 choices, to pattern II. This occurred in both of the test series. Another unusual thing also happened. On the day following test series 1, this animal reverted to pattern I on 9 of his 10 runs. Since the animals were not run for a week after the conclusion of experiment *A*, we cannot be certain that this same reversion would have occurred after test series 2, but a few runs without shock, directly following test 2, lead us to believe that the same thing would have happened had rat no. 1 been run on the following day. A possible explanation of this will be considered later.

B. The second experiment was designed to determine whether an electric shock outside the apparatus would cause the same reversion precipitated by the shock administered within the apparatus. For this, a small covered box, $5\frac{1}{2}'' \times 6'' \times 12''$, the floor of which was a charged grid, was used. On the day of the test trials, each rat was placed in the box and subjected to an electric shock for ten seconds, then removed directly to the apparatus. The procedure was repeated on the nine succeeding runs of the series. While the situation caused very evident disturbances, the behavior pattern was very little affected by this procedure. None of the animals made more than two of the ten choices in the direction of pattern I, and one animal (No. 4) chose pattern II on all ten runs. Since the lapse of time between the shock and the animal's presence in the apparatus was very short (never more than two seconds), and since the emotional disturbances resulting from the shock were present during all of the time that the animal was in the maze situation (the criteria of this emotionality were those used by Hall (5), defecation, urination, etc.), we may conclude that failure to obtain reversion to pattern I in this situation was not due to the rat's recovering from the effects of the shock so much as it was due to the fact that the disturbing stimulus was outside the apparatus, and not 'bound up' intimately enough with behavior at the choice point to cause any such reversion as

in experiment *A*. Disturbance was undoubtedly present, but it was apparently not so critical or so disrupting as in the previous experiment.

TABLE 3

SHOWING RUNS ON TEST DAY AND DAY PRECEDING. EXPERIMENT *B*

Rat No.		1	2	3	4	5
Choices on day preceding test	Pattern I.....	0	0	0	0	0
	Pattern II.....	10	10	10	10	10
Choices on test day	Pattern I.....	2	2	2	0	2
	Pattern II.....	8	8	8	10	8

C. The problem in this experiment was to see if a loud noise, occurring as the rat approached the choice point (*B*), might also initiate the reversion. For this purpose, an electrically operated bell was suspended about twelve inches above point *B* in the maze. As soon as the animal started along alley *AB*, the bell began to ring, and continued until the rat had made his choice. This procedure was followed for all ten trials of the test series.

TABLE 4

SHOWING RUNS ON TEST DAY AND DAY PRECEDING. EXPERIMENT *C*

Rat No.		1	2	3	4	5
Choices on day preceding test	Pattern I.....	1	1	0	0	0
	Pattern II.....	9	9	10	10	10
Choices on test day	Pattern I.....	10	6	8	0	1
	Pattern II.....	0	4	2	10	9

The runs made by the rats in this test (see Table 4) indicate an aspect of the question also suggested by the first experiment. Two of the animals showed a definite reverting tendency, two others showed no disruption in their behavior, and the runs of the fifth rat (No. 2) were divided between the two patterns in what may be regarded as a chance split. To the experimenter, this data, plus the reversion of rat no. 1

on the day following the test series in experiment *A*, suggests the possibility that a very delicate equilibrium is maintained between the two modes of response offered by the situation, and that this balance may be easily upset.

D. The fourth experiment was to determine whether injections of caffeine would have a disrupting effect on the rats' behavior. For four days just preceding the test trials, the animals were given .5 cc. of physiological salt solution by subcutaneous injection directly before they were placed in the apparatus. This was to make certain that any disturbances which might be found could be ascribed to the caffeine and not to the hypodermic. On the fifth day, each rat was given .25 mg. of caffeine per kilogram of body weight, ten minutes before he was placed in the maze situation, and on the sixth day, the dosage was increased to .5 mg. per kilogram of body weight. As Table 5 shows, there was no observable difference in the runs on those days as compared with the day just preceding.

TABLE 5

SHOWING RUNS ON TEST DAY AND DAY PRECEDING. EXPERIMENT *D*[3]

Rat No.		1	2	3	4
Choices on day preceding test	Pattern I.....	0	0	0	0
	Pattern II.....	10	10	10	10
Choices on test day 1	Pattern I.....	0	1	1	0
	Pattern II.....	10	9	9	10
Choices on test day 2	Pattern I.....	0	0	0	0
	Pattern II.....	10	10	10	10

[3] Rat no. 5 was not subjected to this experiment as he had not reached the criterion again after the previous test. Since no disturbances were noted in the behavior of the other rats, it was not considered necessary to test him in this situation.

E. Since among the effects of adrenaline is a stimulation of the receptive mechanism of the sympathetic system, it was thought that possibly adrenaline injections might produce an emotional disturbance like that found with electric shock and in some cases with noise. To this end, subcutaneous injec-

tions of adrenaline (epinephrine hydrochloride) were given, preceded of course by injections of salt solution on three days. On the first test day, the rats were given a hypodermic of .05 cc. of Parke & Davis 1 : 1000 adrenaline and .45 cc. of salt solution. On the second test day, the injections were .5 cc. P. & D. 1 : 1000 adrenaline. As Table 6 shows, neither

TABLE 6

SHOWING RUNS ON TEST DAY AND DAY PRECEDING. EXPERIMENT *E*

Rat No.		1	2	3	4	5
Choices on day preceding test	Pattern I.....	0	1	0	0	0
	Pattern II.....	10	9	10	10	10
Choices on test day 1	Pattern I.....	0	0	0	0	0
	Pattern II.....	10	10	10	10	10
Choices on test day 2	Pattern I.....	0	1	0	0	0
	Pattern II.....	10	9	10	10	10

of the injections had any behavioral effects. No physiological effects (*e.g.*, blanching of ears and nose, increased heart rate, etc.) were observable after the first injection, and only a very slight blanching of the ears was present after the second. While it may be argued that the absorption from subcutaneous injections is so delayed by the vasoconstrictor effects of adrenaline that the response is quite slight, we may consider that, with such a large dose as was administered on the second day, enough would have been absorbed to produce some reaction. We must therefore assume that adrenaline did not in this case produce emotional disturbance sufficient to cause an upset such as that caused by electric shock.

F. This experiment is virtually a repetition of *A*, except that the amount of practice on pattern II in proportion to that on pattern I is greatly increased. As Table 7 shows, at the time of this test series, pattern II was a great deal stronger than pattern I, from the standpoint of the frequency with which it had been practiced; furthermore, since the time when pattern I had been practiced (*i.e.*, had been the correct re-

sponse to the situation), pattern II had been overlearned. Thus also from the standpoint of recency pattern II was dominant. The only factors favoring pattern I were its primacy and its nativeness, both of which might be regarded as doubtful benefits when compared with the factors of frequency and recency supporting pattern II. Pattern I had been the wrong choice for at least 500 training trials just preceding this test, and if we were to count all of the test trials inserted, some of which had no observable effects on the rats, and which served in all probability merely as further training runs, our total of uninterrupted practice runs on pattern II would be quite a bit greater. On the basis of training runs only, from the conclusion of the first experiment to the beginning of the present one, we have very definite evidence of what would constitute overlearning of such a simple habit as a single turning response.

Test trials followed the same procedure as in experiment *A*, but only one test series was given. A consideration of Table 7 shows that in spite of this increased practice on

TABLE 7

SHOWING TOTAL TRAINING RUNS (EXPERIMENTS *A–E*), CHOICES IN TWO PATTERNS DURING ALL PATTERN II RETRAINING SERIES, AND CHOICES ON TEST DAY AND DAY PRECEDING. EXPERIMENT *F*

Rat No.		1	2	3	4	5
Total number training runs	Pattern I.....	420	310	360	420	430
	Pattern II.....	570	530	500	500	530
No. choices in patterns during pattern II retraining series after experiment *A*	Pattern I.....	10	4	1	9	2
	Pattern II.....	171	156	179	181	128
Choices on day preceding test	Pattern I.....	0	0	0	0	0
	Pattern II.....	10	10	10	10	10
Choices on test day	Pattern I.....	7	10	10	9	10
		3	0	0	1	0

pattern II, the shock caused a sudden and almost complete reversal to pattern I. Furthermore, in cases where this re-

version was not 100 percent, the choices in the direction of the second pattern (in this and in other experiments where a reversal occurred) were always the first choice or choices in the series. In other words, if L = pattern I and R = pattern II, the ten runs of rat no. 1 on the test series were RRRLLLLLLL, in that order. This fact might possibly shed some light on the problem of how regression occurs.

G. This experiment was planned to test the adequacy of the conditioning explanation of the phenomenon isolated in these experiments. As we said before, the reversion which was obtained under some of the conditions described above might very well be explained as disinhibition, or the release of a reflex from inhibition (here, of delay), this release being the result of extraneous stimuli. If this were a satisfactory explanation, by retraining the rats in pattern I and by then giving an electric shock on grid *AB*, we would expect a reversion to pattern II, an inhibition to which was built up as the result of the longer delay now following a choice in pattern II.

To this end, the rats were retrained in pattern I directly following experiment *F*, and were then given an electric shock, as in experiments *A* and *F*. By the time the criterion had been reached, the number of choices in the two habits during training trials was nearly equal, and in no case did runs in pattern I exceed those in II by more than a very few trials. Further, since we found that a very definite reversion occurred in experiment *F*, which was directly preceded by an overlearning of the second pattern, and which took place when the total number of training runs was 'loaded' in favor of the second pattern, we feel justified in expecting a reversal of pattern to occur as successfully here as in the previous experiment, provided we consider the conditioning explanation adequate. But if we believe that the phenomenon we have found here is regression as we define it, a reversion to behavior pattern II would invalidate our reasoning. Regression, by our definition, must be to an earlier mode of behavior, independent of the adequacy of that response in the situation. Whether this earlier mode of response must ontogenetically precede the second mode, or whether both may merely be

learned responses, are questions which will be considered later; the experiment at hand is to determine whether the shock caused disinhibition or regression, as those terms are here defined.

Table 8, besides presenting data on the total choices made

TABLE 8

SHOWING TOTAL TRAINING RUNS, TOTAL CHOICES MADE, CHOICES ON TEST DAY AND ON DAYS PRECEDING AND FOLLOWING. EXPERIMENT *G*

Rat No.		1	2	3	4	5
Total number training runs		1090	1110	1060	1240	1150
Total number of choices made	Pattern I	405	577	426	616	571
	Pattern II	685	533	634	624	579
Choices on day preceding test	Pattern I	10	9	9	9	10
	Pattern II	0	1	1	1	0
Choices on test day	Pattern I	10	10	10	10	10
	Pattern II	0	0	0	0	0
Choices on day following test day	Pattern I	10	10	9	8	9
	Pattern II	0	0	1	2	1

during the experiments, also gives the individual records on the test day and on the days just preceding and following.[4] We can see very definitely that there was no reversal to pattern II, as we would expect if disinhibition took place. We have instead a complete fixation upon pattern I. The same emotional disturbance was present during this series as during the shock trials of earlier experiments, but we may infer that there was no change in the direction of running because the rats had nothing earlier to which they might revert, unless it was to cessation of all movement (see Wells (**15**), p. 857). This very thing did occur moreover, in the case of rats no. 1 and 5, in several of their trials.[5] When these rats refused to go on in the maze situation for four minutes or more, it was

[4] The records are included for the day following the test series to demonstrate that no reversion took place here as in the one instance during experiment *A*.

[5] This happened on 7 runs of no. 1 and 9 runs of no. 5.

necessary to remove them, but they were, on each occasion, given a nibble of food before being again placed in the maze, to keep the reward situation constant. Also, they were never removed before they had made a choice at *B*, so that we have a complete record of the choices on these trials. All of this evidence gives credence to the experimenter's belief that what occurred was regression due to emotional disturbance, and not disinhibition.

IV. Results and Discussion

Before going on to a consideration of the results obtained, the writer wishes to point out that while the actual numerical values obtained may not be reliable because of the small number of animals used, the tendencies shown are far above chance. Further, the study of regression must necessarily be the analysis of the records of individual rats, for this phenomenon is apparently an emotional thing, and has specific effects upon the different animals. Thus because of the uniqueness of the phenomenon the experimenter feels justified in making tentative statements even on the basis of so few cases.

Many things which occurred during the experiments tend to postulate this uniqueness. Let us consider rat no. 1 first. On the day of the first test series in experiment *A*, he showed no tendency toward reversion, while on the day following the test series, he reversed so that 90 percent of his choices were in the direction of pattern I. He also showed evidence of repeating this on trials subsequent to the second test series. His RRRLLLLLLL series in experiment *G* might be interpreted that the effects of the shock were cumulative, rather than immediate. This, together with the very divided results obtained using the bell as the disturbing stimulus, suggests that there is a certain specificity of action about regression in individual cases.

We do not wish to dwell upon exceptional cases which occurred, but the implications which they make are too important to be overlooked. The facts cited above indicate that the relation of the two patterns is a very dynamic sort

of equilibrium, the balancing factors of which have been studied too little thus far.

The failure to obtain regression under the conditions of experiments *B*, *D*, and *E*, and the partial failure in experiment *C* also present very interesting possibilities. In the case of *B*, we may consider that the disrupting stimulus was not enough a part of the response situation to be critical, or that the law of contiguity was violated with regard to space at least, if not also to time. Experiment *C* suggests the part that individual differences may play in regression. It is quite reasonable to expect that an arbitrarily chosen disrupting stimulus would not have the same effect upon all animals. This is fully borne out by a qualitative analysis of the reactions of the various animals used. Those rats that maintained pattern II as the dominant one in the situation showed far less 'fear' and 'anxiety' toward the novel factor than did those reverting to pattern I. Also, they spent more nearly the normal amount of time in running the series, and showed little of the hesitation in entering or leaving the delay box manifested by the regressing rats. With regard to experiments *D* and *E*, the administration of stimulants, especially adrenaline, might be expected to cause disturbance, and the fact that it did not may be interpreted two ways: either the process of absorption was so retarded that no response was discernible; or the stimulation was present, but was not 'critical' enough to produce a behavioral disturbance. Of these two, the writer is in favor of the second explanation.

The results of experiment *F* tend to invalidate the claims of other experimenters on the problem, that the greater strength of the second pattern due to practice causes fixation on that habit instead of regression to a previous one. This idea is offered by Martin (**10**) as a possible reason for his failure to obtain regression, by Krechevsky and Honzik (**9**), who suggest that perseveration is a result of greater frequency in the opposite path, and by Hamilton and Krechevsky (**8**), who, in training their animals, purposely made pattern I stronger than pattern II from point of frequency,[6] to facilitate

[6] Hamilton and Krechevsky trained the rats to a high criterion in pattern I, and in pattern II only until the animals showed a *tendency* to adopt it (12 turns in the direction of pattern II out of 50 consecutive free runs).

the occurrence of regression. But in the present study, regression occurred from a habit which had been overlearned, to one which had been 'learned' only once. The best explanation of this is that the first pattern was one for which the organism had a genetic bias, while the second was in the opposite direction from that bias, and that the genetic factor somehow makes a pattern stronger irrespective of practice. A critical test of this idea would be to train animals from the beginning in the pattern opposite to their natural preference, to see if shock produced a fixation upon the learned pattern or a reversion to the unlearned. A study of this sort, together with the evidence offered in the present experiment that any reversion resulting would be regression and not disinhibition, would present crucial data to answer this question.

Experiment *G* substantiates the belief that the animal regresses to a behavior ontogenetically of a lower level. The fact that the rats kept on running in pattern I may be interpreted to mean that they had no lower level to which they might regress, and consequently fixated upon the pattern with which they had been responding. The fact that some of the rats refused to run at all may be because cessation of all activity, or 'freezing to the spot,' is a still lower level of response than the natural turning preference. By this reasoning, these were cases of regression also. The rats had no earlier active response in the situation to which they could revert, so they went 'from a more specific hypothesis to a more general means-end-readiness,' which is Tolman and Krechevsky's definition of regression (**14**).

The advantage of having a simple situation, although questioned by Martin (**10**), seems to the experimenter to be quite apparent in the light of the results. Martin used the hypothesis concept of Krechevsky, and also the same apparatus, an insolvable multiple discrimination box. The animal chose an hypothesis, and then a constant pattern was used, which employed cues the opposite to those preferred by the rat in his first hypothesis. In view of Martin's failure to demonstrate regression to the first hypothesis, we may tentatively conclude that this 'unit' of behavior is not genetically

characteristic of the organism. The extremely dynamic nature of the hypothesis would lead us to suspect that this explanation is in part correct. It is known that the hypothesis would soon have been abandoned for another had no change been introduced in the insolvable situation. Furthermore, it may be that the complications found in the hypothesis situation would be such as to obscure any regressing tendency which might be present. The most probable reason, however, is that a simple turning preference would more than likely be the sort of thing regressed to; its very simplicity would recommend it. Martin criticizes the use of a maze with only two alternatives on the grounds that the regressive responses would be due to 'a reaction against the other alley (rather than) to an actual positive attraction of the former habit.' The writer feels that this argument has been eliminated by experiment *G*, in which the response to shock was a fixation on the pattern employed just previous to shock, and thus definitely not an avoidance reaction.

A further explanation of Martin's failure to isolate the phenomenon in question may have been in the way the shock was given. As we know, no regression resulted from the administration of a shock outside the apparatus, presumably not because of the strength of the shock (which was the same as in all shock series), but because it was not a part of the situation. Now in Martin's experiment, the rats were shocked in a starting box shut off from the apparatus. A door was then opened, and the animals entered the first unit. There is no reason why this procedure should be successful any more than in the case of experiment *B*. True, there was an emotional disturbance, but there is every reason to believe that it was not what we have called a 'critical' disturbance, just as in experiment *B*.

V. Conclusions

Our results here may in general be summed up in the following conclusions.

1. Regression analogous to the emotional phenomenon in humans has been isolated in the rat.

2. When it occurs, this regression is usually a sudden complete reversion to an earlier mode of response, more genetically

characteristic of the organism than the behavior regressed from.

3. Regression is not adaptive or docile (*i.e.* the response regressed to persists despite its relative ineffectiveness in solving the problem confronting the organism).

4. Regression has an emotional basis, and this emotionality must be very closely integrated with the situation.

5. Regression may occur quite independently of the relative amount of practice on the two behavior patterns (it is not frequency or recency which makes one pattern dominant over the other).

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References

1. Elliott, M. H., The effect of hunger on variability of performance, *Amer. J. Psychol.*, 1934, **46**, 107–112.
2. Everall, E. E., Perseveration in the rat, *J. Comp. Psychol.*, 1935, **19**, 342–369.
3. Gengerelli, J. A., An experiment on the natural preference of rats for right and left turns, *J. Comp. Psychol.*, 1930, **10**, 263–265.
4. Gilhousen, H. C., An investigation of 'insight' in rats, *Science*, 1931, **73**, 711–712.
5. Hall, C. S., Emotional behavior in the rat—I: defecation and urination as measures of individual differences in emotionality, *J. Comp. Psychol.*, 1934, **18**, 385–403.
6. Hamilton, J. A., and Ellis, W. D., Behavior constancy in rats, *J. Genet. Psychol.*, 1933, **42**, 120–139.
7. ——, Persistence and behavior constancy, *J. Genet. Psychol.*, 1933, **42**, 140–153.
8. —— and Krechevsky, I., Studies in the effect of shock upon behavior plasticity in the rat, *J. Comp. Psychol.*, 1933, **16**, 237–253.
9. Krechevsky, I., and Honzik, C. H., Fixation in the rat. *Univ. Calif. Publ. Psychol.*, 1932, **6**, 13–26.
10. Martin, R. F. *An attempt at the experimental demonstration of regression in hypotheses in rats.* (Unpublished M.A. Thesis, U. of Oregon, 1937.)
11. Pavlov, I. P. *Conditioned reflexes:* an investigation of the physiological activity of the cerebral cortex. London: Oxford Univ. Press, 1927. Pp. 336.
12. Sanders, M. J. *Fixation in the rat.* (Unpublished.)
13. Skinner, B. F., A failure to obtain disinhibition, *J. Gen. Psychol.*, 1936, **14**, 127–135.
14. Tolman, E. C., and Krechevsky, I., Means-end-readiness and hypotheses—a contribution of comparative psychology, *Psychol. Rev.*, 1933, **40**, 60–70.
15. Wells, F. L., Social Maladjustments: Adaptive Regression, *Handbook of Social Psychology*, ed. C. Murchison, Worcester, Mass.: Clark Univ. Press, 1935, Ch. 18.
16. Yoshioka, J. G., A note on the right or left going position habit with rats, *J. Comp. Psychol.*, 1928, **8**, 429–433.
17. ——, A further note on a position habit in rats, *J. Comp. Psychol.*, 1930, **10**, 309–315.

AVOIDANCE OF REPETITION OF JUDGMENTS ACROSS SENSE MODALITIES

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Introduction

That serial judgments are not wholly independent of one another was first shown by Fernberger [1] who gave a technique which compensated for the effects of the dependence in the results of psychophysical experimentation. Arons and Irwin,[2] using the equal weight technique discovered that their Ss tended to avoid repeating judgments of the same category, a fact quite similar to certain of Fernberger's observations. Later experiments by Preston [3, 4, 5] demonstrate the existence of avoidance of repetition: 1, in the case of judgments separated by as many as three intervening judgments; 2, in the case of judgments separated by empty temporal intervals as long as 11 seconds; and 3, in psychometric functions developed from judgments each of which followed a judgment on two equal weights.

Preston was disposed to regard this phenomenon as independent of stimuli and sensory processes. That the existence of the tendency to avoid repetition is not determined by stimulus variation follows from the fact that equal weights were used; that it is not a consequence of sensory factors is made probable by the fact that it persists even over a series of intervening judgments. In addition, the avoidance of the

[1] S. W. Fernberger, Interdependence of judgments within the series for the method of constant stimuli, *J. Exper. Psychol.*, 1920, **3**, 126–150.

[2] L. Arons and F. W. Irwin, Equal weights and psychophysical judgments, *J. Exper. Psychol.*, 1932, **15**, 733–751.

[3] M. G. Preston, Contrast effects and the psychophysical judgments, *Amer. J. Psychol.*, 1936, **48**, 389–402.

[4] M. G. Preston, Contrast effects and the psychometric functions, *Amer. J. Psychol.*, 1936, **48**, 625–631.

[5] M. G. Preston, The effect of interpolated time intervals upon the contrast effects, *J. Exper. Psychol.*, 1936, **19**, 707–715.

repetition of equal judgments creates serious difficulties for the only theory which attempts to explain the effects of one judgment upon another by means of changes in sensory processes, namely that of Turner.[6]

The present experiment has as its object the identification of the process involved in the avoiding of the repetition of judgments. The results of the previous experimentation reported by Turner, Arons and Irwin, and Preston might conceivably be explained by changes in the intensive magnitude of the sensory processes themselves, although Preston's experiments have made such an explanation difficult. In this experiment we make the assumption that the intensity of a sensory process in one modality affects the intensity of a sensory process in another modality neither to the large extent necessary to account for the changes in threshold values attributable to avoidance of repetition, nor in the partially inverse manner which is characteristic of the avoidance effect. This assumption seems well-grounded in view of the difficulty that has been experienced in establishing very definite cross-modality effects even when the stimuli are simultaneous (see, *e.g.*, Hartmann [7] and Kravkov [8]). The avoidance of repetition, on the other hand, is easily observed, although the two stimuli are separated by several seconds. If then the assumption be granted, the experimental demonstration that judgments on material presented in one sense modality predispose the *S* to avoid the repetition of the same category in making judgments on material presented in another modality will prove that the avoidance of the repetition of judgments cannot be attributed to changes in the sensory processes themselves.[9]

[6] W. D. Turner, Intra-serial effects with lifted weights, *Amer. J. Psychol.*, 1931, 43, 1–25.

[7] G. W. Hartmann, The facilitating effect of strong general illumination upon the discrimination of pitch and intensity differences, *J. Exper. Psychol.*, 1934, 17, 813–822.

[8] S. V. Kravkov, Changes of visual acuity in one eye under the influence of the illumination of the other or of acoustic stimuli, *J. Exper. Psychol.*, 1934, 17, 805–812.

[9] Our study of cross-modality effects must be distinguished carefully from such work as Von Hornbostel's study of cross-modality judgments. Von Hornbostel required his *S*s to make judgments upon an attribute common to two modalities (bright-

It might be argued that in experiments of this kind the *S* is avoiding the repetition of the category names by means of which his response is reported. In order to rule out this possibility it is only necessary to require alternating comparisons in two modalities, stipulating that the report in the first modality be given in one set of terms and the report in the second modality in a second set of terms. The persistence of the avoidance of repetition of reports of equivalent meaning (*i.e.*, louder-heavier, or same-equal) will demonstrate that the avoidance tendency cannot be explained on the basis of the avoidance of particular words themselves.

In general then the purpose of the present experiment is to demonstrate that the avoidance of repetition is not a consequence of changes in the magnitude of sensory processes themselves, nor of some inability on the subject's part to repeat a given word.

Method of Procedure

Three *S*s were used, all undergraduate students at the University of Pennsylvania, with no experience in making comparisons, either on lifted weights or sound intensities. They will be referred to hereafter as Le, Ma and Lr. They were compensated for their work. The *S* sat before a lifted weight turntable arranged in the manner described by Arons and Irwin.[10] Behind him was arranged a table on which rested an apparatus for the production of sounds of uniform intensity. The back of the *S*'s chair was a constant distance of 140 cm. from the sound-producing apparatus. The apparatus used to produce the sound stimuli consisted of an ebony block 15.6 cm. × 5.1 cm. × 4.8 cm. fixed securely beneath an electromagnet. By means of an electrically driven metronome (located in a distant room) the circuit of the electromagnet was broken 23 times per minute. In this fashion a steel ball-bearing weighing 8.35 gm. and being 13 mm. in diameter could be dropped from the height of 29.9 cm. to the surface of the ebony block. The sound produced by this method was a sharp click without noticeable resonance. No measures were taken to determine the physical intensity of the sound, but every precaution was observed to maintain it at a constant intensity.

The ball rebounded silently into a felt basket by reason of a slight inclination of the block. An additional circuit breaker on the metronome flashed a small electric light before the *S*'s eyes at a rate of 92 flashes per minute. Each breaking of the circuit in the magnet synchronized with every fourth flash of the bulb. In order to shield the *S*'s eyes from glare the bulb was covered by a piece of gauze. This signal

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ness of grays, tones and odors). We required our *S*s to make alternate judgments of sounds and weights without their being at all concerned with the comparison of any attributes common to the two modalities. For Von Hornbostel's work, see Ueber Geruchshelligkeit, *Arch. f. d. ges. Physiol.*, 1931, **227**, 517–538.

[10] L. Arons and F. W. Irwin, *op. cit.*, page 733.

light maintained the rhythm in which the weights were lifted by the *S* and in which the ball was inserted in the magnet at the proper moment by one of the experimenters.

All of the weights used were held constantly at 100 gm., while all sound intensities likewise were maintained physically equal by the method just described. The *S*'s were kept in ignorance of the fact that both sound and weight stimuli were objectively equal and also of the purpose of the experiment.

The *S*s were first practiced in comparing pairs of sound intensities, the sounds occurring at intervals of four flashes of the signal light. They were practiced at a later day in making judgments on weights presented uniformly in rhythm at 92 flashes per minute. Following these practice sessions a further practice session occurred in which the stimuli were presented to the *S* in alternate pairs, a judgment on weights being required first, a judgment on sounds immediately next, a judgment on weights next and so on until 56 judgment's had been made alternately on sounds and weights. 336 judgments were made during one experimental hour. The following instructions were given the *S*s:

"You will be presented with a series of sounds and weights in alternate pairs. First you will hear a pair of sounds, then you will lift a pair of weights, then you will hear another pair of sounds and so on. You will judge whether the second sound of each pair is louder than, the same as, or softer than the first, using the words *louder*, *same* and *softer* in your reports. You will judge whether the second weight of each pair is heavier than, equal to or lighter than the first, using the words *heavier*, *equal* and *lighter* in your reports."

At the end of four sessions, the instructions were changed. It was intended to investigate by this change in instructions the extent to which the avoidance of repetition depended upon the use of particular category names. The new instructions were as follows:

"You will be presented with a series of sounds and weights in alternate pairs. First you will hear a pair of sounds, then you will lift a pair of weights, then you will hear another pair of sounds, and so on. In either case you will judge whether the second impression of each pair is greater than, equal to, or less than the first, using the words *greater*, *equal* and *less* in your reports. In other words, you are to use the word *greater* instead of the words *louder* and *heavier; equal* instead of *same* and *equal*, and *less* instead of *softer* and *lighter*."

The data were treated in three ways: 1, exactly as they were taken from the *S*, these being called the Sound-Weight data; 2, the judgments on sounds were treated, disregarding intervening judgments on weights, these being called the Sound-Sound data; and 3, the weight judgments were treated independently, disregarding the intervening judgments on sound, these being called the Weight-Weight data.

The determination of the expected number of sequences in all series follows the method given by Arons and Irwin.[11] A slight modification of this method was used in the calculation of the expected number of judgment sequences in the Sound-Weight Series because of the fact that differing proportions of judgments were given as *softer* or *lighter*, *same* or *equal* and *louder* or *heavier*, respectively. The calculation of the expected *lighter-louder* sequences, for example, was accomplished by taking the product of the proportions of *lighter* judgments and of *louder* judgments instead of the square of the proportion of judgments of either *lighter* or *louder*.

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[11] L. Arons and F. W. Irwin, *op. cit.*, pages 738–741.

Results

The results of this experiment are shown in Tables I–IV. Table I gives the value D/σ and D/Calc. for the three sequences involving repetition in the Sound-Weight data. Results are presented in both kinds of series, namely when

TABLE I

Showing D/σ and D/Calc. for all Sequences Involving Repetition in Sound-Weight Series

Seq.	Sound-Weight Different Names						Sound-Weight Same Names					
	Ma		Lr		Le		Ma		Lr		Le	
	D/σ	D/Calc.	D/σ	D/Calc.	D/σ	D/Calc.	D/σ	D/Calc.	D/σ	D/Calc.	D/σ	D/Calc.
LL....	−1.26	−.12	−1.48	−.36	.72	.09	−1.12	−.14	−1.92	−.38	− .52	−.09
EE....	.00	.00	.43	.02	− .74	−.04	−1.66	−.13	.38	.02	−1.25	−.09
GG....	−1.06	−.06	1.17	.09	−1.39	−.11	−2.83	−.22	.13	.01	−1.90	−.13

TABLE II

Showing D/σ and D/Calc. for all Sequences Involving Repetition in Sound-Sound Series

Seq.	Sound-Sound Different Names						Sound-Sound Same Names					
	Ma		Lr		Le		Ma		Lr		Le	
	D/σ	D/Calc.	D/σ	D/Calc.	D/σ	D/Calc.	D/σ	D/Calc.	D/σ	D/Calc.	D/σ	D/Calc.
LL.....	− .10	−.01	−.96	−.49	.11	.02	.39	.05	− .01	−.003	.08	.002
EE.....	−1.83	−.27	−.27	−.02	− .72	−.05	−.61	−.08	−1.02	−.08	.92	.10
GG.....	− .05	−.04	−.54	−.004	−1.11	−.15	.79	.10	− .27	−.03	.38	.04

TABLE III

Showing D/σ and D/Calc. for all Sequences Involving Repetition in Weight-Weight Series

Seq.	Weight-Weight Different Names						Weight-Weight Same Names					
	Ma		Lr		Le		Ma		Lr		Le	
	D/σ	D/Calc.	D/σ	D/Calc.	D/σ	D/Calc.	D/σ	D/Calc.	D/σ	D/Calc.	D/σ	D/Calc.
LL.....	−1.36	−.25	.23	.05	−1.23	−.23	.77	.18	−.27	−.07	−2.56	−.63
EE.....	−1.24	−.21	− .69	−.04	−2.44	−.20	−.30	−.04	.00	.00	− .82	−.09
GG.....	−1.76	−.10	−1.02	−.14	−2.28	−.23	−.31	−.03	.22	.04	−1.67	−.16

TABLE IV

SHOWING VALUES OF D/σ AND D/CALC. FOR ALL SERIES FOR ALL Ss, BASED UPON THE TOTAL NUMBERS OF CALCULATED AND OBSERVED REPETITIONS

Series	Subject	Different Names		Same Names	
		D/σ	D/Calc.	D/σ	D/Calc.
S-W	Le	−1.2	−.044	−2.7	−.110
	Lr	.8	.025	− .2	−.008
	Ma	−1.6	−.061	−3.9	−.172
S-S	Le	−1.2	−.061	1.0	.061
	Lr	− .8	−.036	−1.1	−.063
	Ma	−1.1	−.058	.4	.024
W-W	Le	−4.1	−.215	−2.7	−.172
	Lr	−1.1	−.052	.0	.001
	Ma	−2.8	−.130	− .2	−.011

the same category names were used for sounds and weights and when different category names were used for sounds and weights. The first column headed *Seq.* reports the three sequences of repetition, LL, EE and GG. L stands for the category of judgment referring to lesser intensity; E stands for the category of equal intensity, and G refers to the category of judgment pertaining to greater intensity. Table I is divided into two sub-tables, one reporting the results of the procedure using different category names, the other reporting the results of the procedure using the same category names. Each sub-table is divided into three main columns, one for each subject. For each *S* two sub-columns give the value D/σ (the ratio of the difference between observed and expected frequencies to the standard error of the expected frequency) and the value D/Calc. (the ratio of that difference to the expected frequency itself). The value D/σ gives a measure of the reliability of the difference, while the value D/Calc. gives a measure of the relative size of the difference. Negative values indicate that fewer sequences were observed than were expected. Positive values indicate that more sequences were observed than were expected. For example, the value − 1.26 appears in the second column of Table I. This means that in the case of subject Ma the critical ratio for the repetition of *less* judgments was 1.26 and that fewer

judgments were observed than were expected. In the third column appears the value — .12. This means that for the same *S* a deficit of *lighter* judgments amounting to 12 percent of the expected number was observed after a judgment of *lighter*. The same interpretation is to be given to the data presented for the other *S*s and for the other sequences. Table II gives the same kinds of values for sequences involving repetition in the Sound-Sound data. Table III gives the corresponding values for sequences of the Weight-Weight data. In every case Tables II and III are to be interpreted as was Table I.

In further summary of the results, Table IV is presented. In this table the values of the three different sequences represented in Tables I, II and III have been consolidated in order to determine whether there has been a general tendency to avoid the repetition of judgments irrespective of the particular category. The first column indicates the series, and the second column the *S*. The third column gives the values of the critical ratios when different names were used; these are the ratios of the differences between the calculated and observed numbers of repetitions to the standard error of the frequency expected. They are similar in meaning to the critical ratios of Tables I, II and III, except that they are based upon the total number of expected and observed repetitions instead of upon the numbers expected and observed in one category. The fourth column gives the ratio of the differences between the calculated and observed numbers of repetitions to the calculated number; these values also are based upon the totals for the three categories taken together. The fifth and sixth columns give the same information for the series using the same category names.

Summary of Results

1. It is recognized that certain of the results presented in the tables are indeterminate in the case of some *S*s and in the case of some series.

2. Examination of Table I indicates that avoidance is present for two *S*s using different names and for the three *S*s using the same category names.

3. In Tables II and III, treating Sound-Sound and Weight-Weight data respectively, using different category names, all three *S*s show avoidance of repetition.

4. Table IV discloses the fact that when the three categories are combined to give a single value, avoidance of repetition appears in much the same way as it appeared in the previous analysis, where the categories were considered separately.

Discussion

As stated above the purpose of this experiment is to determine, first, whether or not the tendency to avoid repetition of judgment is a reflection of genuine changes in the magnitude of sensory processes, and second, whether or not the avoidance tendency is dependent upon the mechanism involved in the giving of the response words themselves. We consider this question to be important for the following reasons:

1. The influences being measured are below the level of the *S*'s consciousness. This fact is indicated by the following reports of the *S*s, made at the conclusion of the experiment when they were asked, first, whether or not they had observed any particular order in the appearance of the stimuli, and second, what they thought was the purpose of the experiment. Lr reported:

"No, there seemed to be no definite order; sometimes the 'greater' would predominate, and sometimes either the 'less' or 'equal' would appear most. I did think though, that the judgments appeared or occurred in groups as above. I thought that the purpose might be to test for auditory fatigue, and then, when both weight-lifting and auditory tests were taken simultaneously, I believed it might be for the purpose of determining the relation, if any, of auditory and muscular discrimination or fatigue. During the fourth and fifth (first few of the weights and sounds) I tried to predetermine what the next judgment was likely to be. I assumed that there would be a likely distribution of judgments (*e.g.*, after a 'greater' or 'heavier' a 'less' or 'softer' would follow). However sometimes this did not hold true, as mentioned before, so I finally gave up the idea of trying to

foretell the next judgment. This was about the sixth or seventh test."

Ma reported:

"Although it occurred to me that there might be an order in the experiment it did not seem as if any one order carried all the way through. No order seemed to last even through one testing. It seemed to me that the experiment was to serve various purposes. Firstly, a comparison between the fineness of differentiation between two series. Secondly, the result of continued practice. Thirdly, the result of any fatigue that might set in."

Le reported:

"At first I thought there might have been a correlation between heavier weights and louder sounds, etc., but after a time this idea was removed. I believe the experiment had as its purpose a determination of discrimination ability of the subject. Also the effect of continued practice in the discrimination as well as the effect of rhythm in this ability."

The only indication in these reports of the possible appearance of a conscious avoidance of repetition is Lr's statement of his "assumption that there would be a likely distribution of judgments, etc." which is immediately followed by his statement that he discarded this notion.

Further support of our contention in favor of the unconscious nature of the tendencies is found in the fact that they exist, as seen in the results in both the Sound-Sound and Weight-Weight series, where a judgment intervened between the two judgments constituting a sequence; and the fact that a similar phenomenon has been observed by Preston [12] in the case of judgments separated by as many as three intervening judgments (involving a time interval of 48 seconds). We feel that the quantitative treatment of processes operating below the conscious level is of general interest to psychology.

2. The tendencies here dealt with materially influence psychophysical judgments, even to the extent of producing reversals from what is to be expected in the case of individual comparisons. Furthermore we believe that we are not merely

[12] M. G. Preston, Unpublished results taken in connection with the study of interpolated time intervals and their effect on the contrast effects.

adding one more item to the already lengthy list of factors influencing the psychophysical judgment; but rather that the tendency to avoid repetition has a much wider range of application. Thorndike [13] and Dodge [14] have already indicated the possible extent of the field in which avoidance of repetition may be found.

Consideration of the results summarized above for the Sound-Weight Series when the same category names were used indicates without question that *S*s avoid the repetition of judgments in the same category even though the comparisons affected are made in two different sense modalities. Hence according to the argument in the introduction, the existence of an avoidance tendency not due to changes in the sensory processes is established. Furthermore, when different category names are used, the results present a picture of tendencies which have been weakened but which still exist. We conclude from this that whatever the *S* is avoiding he is not merely avoiding the saying of the same words.

The demonstration that this tendency operates at a level higher than that of sensory processes does not, of course, establish its upper level. The fact that it operates without the *S*s in general being aware of its presence indicates that it undoubtedly operates below the level of conscious ideation. It has already been suggested [15] that the intra-serial effects are due to an attitude assumed by the *S*. This suggestion is not contradicted, but rather supported by the facts just discussed. The avoidance of repetition, since it persists through so many variations of experimental procedure, and since it seems to be an unconscious tendency, agrees well with the general concept of an attitude.

(Manuscript received June 7, 1937)

[13] E. L. Thorndike, The refractory period in associative processes, *Psychol. Rev.*, 1927, **34**, 234–236.

[14] R. Dodge, A note on Professor Thorndike's experiment, *Psychol. Rev.*, 1927, **34**, 237–240.

[15] M. G. Preston, The effect of interpolated time intervals upon the contrast effects, *J. Exper. Psychol.*, 1936, **19**, 714–715.

STUDIES IN THE PSYCHO-PHYSIOLOGY OF TRANSFER. I. THE PROBLEM OF IDENTICAL ELEMENTS

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'Transfer,' or the effect of one task upon another, has been a much debated problem in psychology. Considerable evidence has been accumulated showing improvement in performance following exercise of some other activity. Because of its close association with the doctrine of formal discipline, this positive or 'training' transfer has received the major attention and we have tended to neglect instances of negative transfer, such as the effect of interpolated activities in 'forgetting' and 'fatigue.' It is recognized, however, that the question of whether one act 'transfers' increment or decrement to another depends largely on time factors, and that the phenomena are to be explained by the same basic assumption—namely, a structural-functional relation between the two acts. It is unnecessary to cite the experimental evidence of transfer—either positive or negative—since excellent reviews of this literature are already available.[1] Of the various hypotheses which have dealt with the specific nature of this relation, two seem capable of sufficiently exact formulation to be put to test. These are (1) the theory of identical elements, and (2) the theory of irradiation or 'spread.' The present study deals with an experimental check on the validity of the identical element theory. Later reports will be concerned with tests of the irradiation theory.

The theory of identical elements, usually attributed to Thorndike, suggests that one activity alters another to the degree that both utilize the same neural elements. For 'proof,' however, its proponents commonly resort to a circular argument. Two tasks are said to have identical elements be-

[1] C. W. Bray, Transfer of learning, *J. Exp. Psychol.*, 1928, 11, 443–467.

cause they show transfer. Thus if it is shown that practice in addition causes an increment in subtraction but none in multiplication, it is argued that addition and subtraction activities utilize more identical neural elements than do addition and multiplication. And when evidence supporting the view is called for, we are told that the addition-subtraction combination showed the most transfer!

The chief difficulty in putting the identical element theory to experimental test lies in finding performances which are related to each other in relatively simple and definable ways. Bills and McTeer's [2] study made a beginning in this direction. Their subjects alternated at one minute intervals between a standard and a comparison task involving the writing of alphabet sequences. In the first condition all letter elements were common in both standard and comparison tests (abc-abc). The second condition held only one common element (abc-afe) and the third held no common elements (abc-dfe). In each condition tasks were alternated over a period of 15 minutes so as to build up a cumulative fatigue effect. By counterbalancing, no condition received the benefit of more practice than another in the final averaging. The results offered positive support of the identical element theory, showing that the amount of decrement developed in alternating tasks was proportional to the number of letter elements which they had in common.

It is obvious that Bills and McTeer's use of the term 'element' is very specialized, and, as they point out, would probably not be acceptable to those originally promulgating the doctrine of identity. They do not, and cannot refer their results either to neural elements or stimulus-response elements. This leaves only a logical distinction, of which they say, "It is possible to select logically identical elements in two stimulus-response situations. That is all the present study claims to have done. The results of this study seem to show, however, that the logical distinctions do have a definite psychological significance." [3]

[2] Bills, A. G., and McTeer, W., Transfer of fatigue and identical elements, *J. Exp. Psychol.*, 1932, **15**, 23–36.

[3] A. G. Bills and W. McTeer, *loc. cit.*, p. 36.

We can agree with Bills and McTeer that defining the similarity between tasks in any but superficial logical terms is extremely precarious. It is questionable, however, if their results actually support the identity theory of transfer. It may be, instead, that the higher work output found for the abc-def task is due to the greater ease with which this letter combination can be written. In other words, it seems that Bills and McTeer's correlation involves the questionable assumption that it is as easy to write one unit of the alphabet as it is another unit. Furthermore, while writing d is logically different from writing a, the two acts involve similar movements. The lack of contact between Bills and McTeer's 'elements' and either structural or functional units does not constitute a satisfactory test of the identifying theory of transfer. Their results contribute to the problem at a very complicated level. For instance, increment for abc-def combinations over abc-abc might be due to increased interest and motivation in the less monotonous task. However interpreted, the data are sufficiently challenging to make necessary a retest of the identity theory with psycho-physiological 'elements' rather than logical ones.

The Experiments

In rethinking the theory of identical elements in psycho-physiological terms, it seemed that identity between two tasks might involve either (*a*) the same receptors, (*b*) the same effectors, or (*c*) the same central (adjustor) pattern. This led us to select typewriting as the test performance and to vary the identity of transfer-tasks in regard to each of the three fundamental divisions of the neuro-muscular action sequence. The general plan of our experiments followed that of Bills and McTeer; that is, we alternated a standard task with itself and with others involving a lesser degree of identity, under the assumption that the greater the similarity of tasks the greater the transfer effect. By using subjects well advanced in typing and by properly counterbalancing conditions, we sought to minimize positive transfer effects and to develop decrement in the shift from standard to

comparison task. The subjects were motivated by competition for a high score, and when one factor such as effector identity was being varied, the other parts of the reaction arc (receptors and central 'adjustor' patterns) were held constant. Instructions were:

We are working on a speed test in typewriting. This group has been chosen to help in the task. I shall be here for several days; each time I shall administer a slightly different type of test. You are going to type patterns of letters upon the papers which I shall distribute. In general, you will work for 5 minutes upon each of two different letter patterns with a half minute rest between. Ten minutes work will be placed on each page. I will give the start and stop signals. Get the first page into the machine, and type your name and group number at the top of the page. Be sure that you begin at the very top so that you will have room for the total work. Use single spacing—do not use the spacer in any test. Keep both hands in the conventional position on the machine, whether using them or not. Begin every new line with a complete pattern. If you are in the middle of a pattern when you come to the end of the line, shift your carriage and begin the next line with the *beginning* of the pattern. Don't use your margin release in order to complete a pattern at the end of the line. Have your margins set as narrow as possible to begin with. If you get a pattern wrong do not correct it; proceed instead to type the next pattern.

In the procedures outlined below, the term 'condition' refers to the degree of identity between the alternating tasks and the term 'cycle' refers to the practice order in which the conditions were changed. It will be seen that five experiments were made; *viz.*, one in which the identity of effectors was varied, one in which the identity of receptors was varied, two in which the central (adjustor) pattern was varied, and one in which normative values were obtained for each task.

Procedure 1	Normative Values Established for Various Patterns	45 Subjects; 5 Minutes Each in Following Practice Order: Cycle:				
		I	II	III	IV	V
Pattern 1.....	adsf (fingers 4, 2, 3, 1—left hand)	1	2	3	4	5
Pattern 2.....	fsda (fingers 1, 3, 4, 2—left hand)	2	3	4	5	1
Pattern 3.....	;klj (fingers 4, 2, 3, 1—right hand)	3	4	5	1	2
Pattern 4.....	jl;k (fingers 1, 3, 4, 2—right hand)	4	5	1	2	3
Pattern 5.....	adsf (left hand response to auditory stimulation)	5	1	2	3	4

Procedure 2	Effectors Varied	5 Min. Constant Task	5 Min. Comparison Task
Condition 1.....	All common effector elements	Type left hand adsf	Type left hand adsf
Condition 2.....	No common effector elements	Type left hand adsf	Type jklj right hand
Condition 3.....	Control	Type left hand adsf	Rest

45 subjects; cycle I, cond. 1, 2, 3; cycle II, cond. 2, 3, 1; cycle III, conditions 3, 1, 2.

Procedure 3	Receptors Varied	5 Min. Constant Task	5 Min. Comparison Task
Condition 1.....	All common receptor elements	Type various asdf combinations in left hand as signaled by place location of lights	Type various asdf combinations in left hand as signaled by place location of lights
Condition 2.....	No common receptor elements	Type various asdf combinations in left hand as signaled by place location of lights	Type various asdf combinations in left hand as signaled by place location of taps
Condition 3.....	Control	"	Rest (eyes closed)

45 subjects; cycle I, cond. 1, 2, 3; cycle II, cond. 2, 3, 1; cycle III, conditions 3, 1, 2.

Procedure 4a	Central Pattern Varied	5 Min. Constant Task	5 Min. Comparison Task
Condition 1.....	All common pattern elements	Type adsf, left hand; ;klj, right	Type adsf, left hand; ;klj right
Condition 2.....	One common pattern element	Type adsf, left hand; ;klj, right	Type adsf, left hand; jl;k right
Condition 3.....	No common pattern elements	Type adsf, left hand; ;klj, right	Type fsda, left hand; jl;k right

45 subjects; cycle I, cond. 1, 2, 3; cycle II, cond. 2, 3, 1; cycle III, conditions 3, 1, 2.

Procedure 4b	Central Pattern Varied	5 Min. Constant Task	5 Min. Comparison Task
Condition 1.....	All common pattern elements	Type adsf, left hand; ;klj, right	Type adsf, left hand; ;klj right
Condition 2.....	One common pattern element	Type adsf, left hand; ;klj, right	Type adsf, left hand; jl;k right
Condition 3.....	No common pattern elements	Type adsf, left hand; ;klj, right	Type fsda, left hand; jl;k right

45 subjects; cycle I, cond. 3, 1, 2; cycle II, cond. 2, 3, 1; cycle III, conditions 1, 2, 3.

Procedure 5	Central Patterns Alternated every Min. (after Bills and McTeer)	Ten Minute Comparison Task	Ten Minute Standard Task
Condition 1.....	All common pattern elements	Type adsf, left hand; ;klj right	Type adsf, left hand; ;klj right
Condition 2.....	One common pattern element	Type adsf, left hand; ;klj right	Type adsf, left hand; ;klj right

45 subjects; cycle I, cond. 1, 2, 3; cycle II, cond. 2, 3, 1; cycle III, conditions 3, 1, 2.

Procedure 1

Ability to test experimentally the identical element theory of transfer rests on the assumption that the difference in work output between various tasks is due to the transfer effects developed in their combination. It is possible, however, that the difference might be due to the uneven difficulty of the tasks combined; that is, one typing pattern (adsf) may be inherently more difficult of execution than another (fsda), with the consequence that any task combination involving the latter would show decrement. For this reason it was found desirable to establish 'norms' for each typing pattern used, these providing a 'correction factor' in comparison of scores obtained in the remaining experiments. Scatter diagrams showed a normal distribution of individual scores for each pattern. Table I gives the five minute median score for each pattern, together with the correction factors which may be derived therefrom.

TABLE I

Normative Values for Typing Patterns Used in Transfer Experiments

Pattern	Median Score (45 subjects)	Correction Factor	
1	37.5	−5.90	4.23 (correction factor when receptor is varied)
2	35.89	−4.29	
3	36.87	−5.27	0.83 (correction factor when effector is varied)
4	31.61	0.00	
5	33.27	−1.67	
1 and 3	74.36	−6.86	(correction factor when central pattern is varied)
1 and 4	69.11	−1.61	
2 and 4	67.50	0.00	

It is recognized that the use of these correction factors involves an assumption which is not necessarily true; namely, that the same relative difficulty will be preserved by the

pattern combinations throughout the course of the experiments. Our only check against the possibility that a pattern might become more favored by practice was to put some subjects through the procedures in reverse order. Results for procedures 2, 3, 4 and 5 will be discussed without further reference to this question.

PROCEDURE 2

It was thought that the most obvious point to attack the alleged relation between transfer effect and the identity of alternate tasks was by variation of the effectors utilized. Work by Galdo[4] and others had already indicated that exercise of one bodily part decreased the work output of a functionally related part. We argued, therefore, that according to the identity theory, typing a pattern with the left hand followed by typing of the same pattern in the right hand should show less decrement than typing throughout the same period with the left hand alone. As seen in Table II, however, results were exactly opposite to this assumption. Even when the scores are corrected for the small differences due to the greater ease of typing with the left hand, the task-combination having no effector elements in common showed less work output than the one having all common elements. This reversal might be explained as a relation between the identity of effector elements and positive or 'training' transfer. But the score obtained following the control condition of interpolated rest was the highest of all three, thereby indicating that the interpolated work periods did produce 'fatigue' effects.

TABLE II

SHOWING THE EFFECTS OF CHANGING THE RELATIVE IDENTITY IN EFFECTOR PATTERN BETWEEN 5 MINUTE STANDARD AND COMPARISON TASKS

Condition	Ave. No. Patterns in Standard Task	A.D.	Ave. No. Patterns Typed in Comparison Task	A.D.	Difference
Condition 1. (Common effector elements) 5 minute standard right / 5 minute standard right	Cycle 1—27.2 Cycle 2—268.9 Cycle 3—270.5 Ave.....270.2	30.9 29.6 28.4	Cycle 1—269.4 Cycle 2—266.1 Cycle 3—260.5 Ave.....265.5	34.2 28.9 31.2	−4.7
Condition 2. (No common effector elements) 5 min. stan. rt. / 5 min. compar. stan. left	Cycle 1—265.3 Cycle 2—274.1 Cycle 3—257.4 Ave.....265.6	34.2 30.6 31.8	Cycle 1—218.5 Cycle 2—222.8 Cycle 3—219.1 Ave.....220.1	34.8 22.9 26.8	−45.5
Condition 3. 5 min. stan. rt. / 5 min. rest	Cycle 1—277.2 Cycle 2—275.1 Cycle 3—272.8 Ave.....275.1		Rest	29.1 31.5 20.7	

[4] L. Galdo, Bilateralita motoria degli emisferi cerebrali, *Riv. di psicol.*, 1931, 27, 228–241.

Since the relative scores for the three conditions do not follow the assumptions of the identical element theory, explanation of bilateral transfer in terms of common effectors is called into serious question. It also suggests the advisability for repetition of work similar to Galdo's with a search for more suitable explanations.

Procedure 3

Having failed to demonstrate conclusively any predictable relation between the identity of the effector utilized and the degree of transfer, we next turned to the effects produced by variation of the receptor channel stimulated. With the same 45 subjects as were used in Procedure 1, we alternated typing to visual stimulation with itself (condition 1), with typing to auditory stimulation (condition 2), and with rest (condition 3). The typing was done with the left hand in position over the asdf keys, and the order of pressing the keys was signaled by the serial activation of lights or auditory stimuli. Since the rate of typing was controlled by the timing of the stimulation, errors became the variable. According to identical element theory, one of two possibilities should have been exhibited in the comparison of tasks: (1) the vision-vision combination task should have shown fewer errors than the vision-auditory combination task (positive transfer) or (2) the vision-vision task should have showed more errors than the vision-auditory task (negative transfer). Table III shows, however, no significant difference according to the modality utilized although the comparison tasks in both condition 1 and 2 show improvement over the control condition. One can hardly argue therefore that our test was not extensive enough to develop either practice or fatigue.

TABLE III

Showing Effect on Errors of Changing the Relative Identity in Receptor Pattern between 5 Minute Standard and Comparison Tasks

Condition	Ave. No. Errors on Std. Task	A.D.	Ave. No. Errors on Comp. Task	A.D.	Diff.
Condition 1. (Common receptor elements) 5 min. right std. vision 5 min. right std. vision	Cycle 1—21 Cycle 2—26.3 Cycle 3—19.7 Ave.....22.3	2.7 3.4 2.2	Cycle 1—27.5 Cycle 2—22.6 Cycle 3—17 Ave.....22.4	3.1 2.8 3.3	−.1
Condition 2. (No common receptor elements) 5 min. right std. vision 5 min. right comp. audition	Cycle 1—19.4 Cycle 2—25.3 Cycle 3—20.9 Ave.....21.9	3.7 3.1 2.4	Cycle 1—21.4 Cycle 2—28 Cycle 3—19.1 Ave.....22.8	2.8 4.5 2.7	−.9
Condition 3. (5 min. rest 5 min. std. right)	Cycle 1—23.1 Cycle 2—25.4 Cycle 3—24.2 Ave.....24.2	2.7 3.6 2.8	Rest		

PROCEDURES 4*a* AND 4*b*

With little evidence that identity of either effectors or receptors was basic to transfer, we next studied variations in central (adjustor) pattern. In this experiment subjects typed combination-patterns involving both hands. In the first condition, identity between constant and comparison tasks was secured by typing the same pattern combination in both instances. In the second condition one pattern element was changed in the comparison task by having the subjects type a different pattern of the same letters in the left hand. In the third condition, both pattern elements were changed in the comparison task by shifting to a different typing pattern in both hands. Condition I, II and III were performed six times by all subjects. In the first three cycles (procedure 4*a*) the standard task was performed before the comparison task. In the last three cycles (procedure 4*b*), the comparison task was performed first in order to note its effect upon the standard task.

The results of the experiment are given in Table IV*a* and IV*b*. From these we see that the transfer effects between paired tasks carry no certain 'proof' of the identity theory. When the second task was varied so as to have the same or no pattern elements in common with the standard task, little differential effect was noted. When the situation was re-

TABLE IVa

SHOWING THE EFFECTS OF CHANGING THE RELATIVE IDENTITY IN 'CENTRAL PATTERN' BETWEEN 5 MINUTE STANDARD AND COMPARISON TASKS

Condition	Ave. No. Patterns Typed on Std. Task	A.D.	Ave. No. Patterns Typed on Comp. Task	A.D.	C.S.	Diff. in Ave. Sc.	Diff. in Cor. Sc.
Condition 1.	Cycle 1—209.2	23.0	Cyl. 1—198.1	25.2			
(2 common central	Cycle 2—259.5	23.3	Cyl. 2—238.2	25.6			
elements)	Cycle 3—265.6	25.3	Cyl. 3—254.7	31.8			
5 min. std. right and left							
5 min. comp.—std. right and left	Ave.....244.8		Ave.—230.7		230.7	−10.1	−10.1
Condition 2.	Cycle 2—229.5	30.3	Cyl. 1—222.4	33.3			
(1 common central	Cycle 2—213.1	24.3	Cyl. 2—198.5	23.1			
element)	Cycle 3—235.7	23.6	Cyl. 3—225.7	26.5			
5 min. std. right and left							
5 min. comp.—std. left, varied right	Ave.....226.1		Ave.—215.5		220.5	−10.6	−5.6
Condition 3.	Cycle 1—216.9	20.4	Cyl. 1—208.0	23.1			
(No common central	Cycle 2—222.0	25.8	Cyl. 2—204.1	29.7			
elements)	Cycle 3—204.0	22.4	Cyl. 3—192.7	20.8			
5 min. std. right and left							
5 min. comp. varied right and left	Ave.....214.3		Ave.—201.6		206.6	−12.7	−7.7

TABLE IVb

SHOWING EFFECTS UPON A STANDARD 5 MINUTE TASK PRODUCED BY PRECEDING TASKS OF VARYING DEGREE OF IDENTITY IN CENTRAL PATTERN

Condition	Ave. No. Pat. Typed on Comp. Task	A.D.	Ave. No. Pat. Typed on Std. Task	A.D.	Dif. in Ave. Sc.	Dif. in Cor. Sc.
Condition 1.	Cycle 1—163.7	18.2	Cyl. 1—164.1	21.1		
(2 common central elements)	Cycle 2—159.6	20.8	Cyl. 2—154.5	19.1		
5 min. comp.—std. left and right	Cycle 3—165.8	22.4	Cyl. 3—172.0	14.6		
5 min. std. left and right	Ave.....163.0		Ave.—163.5		.5	.5
Condition 2.	Cycle 1—150.8	18.1	Cyl. 1—171.7	15.3		
(1 common central element)	Cycle 2—140.6	16.9	Cyl. 2—164.5	20.5		
5 min. comp.—std. left, varied right	Cycle 3—139.0	19.8	Cyl. 3—162.0	19.4		
5 min. std. left and right	Ave.....143.5		Ave.—171.1		22.6	27.6
Condition 3.	Cycle 1—132.8	21.9	Cyl. 1—157.1	21.0		
(No common central elements)	Cycle 2—139.3	18.4	Cyl. 2—175.6	19.8		
5 min. comp. varied left and right	Cycle 3—129.8	18.5	Cyl. 3—165.9	19.9		
5 min. std. left and right	Ave.....134.0		Ave.—171.2		32.2	37.2

versed, with the comparison task preceding the standard task, the evidence suggests a negative relation between the amount of transfer and the degree of task identity. According to the identity theory, the same relations should have been developed in both experiments, 4*a* and 4*b*. Survey reveals a difference in trend.

Corrected differences between constant and comparison task in procedure 4*a* are not significant. Differences obtained in procedure 4*b* would be positive support of the identical element theory if the comparison tasks gave *decrement* instead of *increment* in the order indicated. From the standpoint of the theory of identity, the only explanation of the inconclusiveness of the data is that the work period was too short to develop well-defined transfer effects. The next experiment was designed as a check on this possibility.

PROCEDURE 5

The same subjects repeated the task-combinations of procedure 4a, but with alternation of constant and comparison tasks every minute over a total of 20 minutes. In this way, the working time was double that previously used, and the minute by minute alternation followed the exact plan of the experiment by Bills and McTeer

which had shown a relation between relative identity of tasks and fatigue-decrement.

Results given in Table V are in the proper direction according to the theory under test; that is, the greater the identity between alternate tasks, the greater the decrement developed. One may question, however, the significance of this relation since the difference between decrements developed in identical and non-identical tasks is very slight, a total of only 8 points. This is much less than the variability of the scores compared.

TABLE V

SHOWING THE EFFECTS OF CHANGING THE RELATIVE IDENTITY IN CENTRAL PATTERN BETWEEN STANDARD AND COMPARISON TASKS ALTERNATED AT ONE MINUTE INTERVALS FOR 20 MINUTES

Condition	Ave. No. Pat. Typed on Std. Task	A.D.	Ave. No. Pat. Typed on Comp. Task	A.D.	Dif. in Ave. Sc.	Dif. in Cor. Sc.
Condition 1. (2 common central elements) 10 1-min. intervals std. rt. and left alternated with 10 1-min. intervals comp. rt. and left	Cycle 1—278.6 Cycle 2—270.2 Ave.....274.4	23.8 34.7	Cyl. 1—279.5 Cyl. 2—218.4 Ave.—248.9	26.7 36.6	−25.5	−25.5
Condition 2. (No common central elements) 10 1-min. intervals std. left and rt. alternated with 10 1-min. intervals comp. varied rt. and left	Cycle 2—275.6 Cycle 1—271.8 Ave.....273.7	37.2 36.8	Cyl. 2—227.7 Cyl. 1—275.4 Ave.—256.6	32.0 39.5	−22.1	−17.1

DISCUSSION

Of the various attempts to explain transfer effects, both positive and negative, the theory of identical elements has had the most supporters. There have been few attempts to test this theory experimentally, and those which have been made are not crucial. Norcross,[5] in his work on the bilateral transfer effects in ability to place numbers in an adding machine, tried to isolate such elements as ability to read the number lists accurately, and familiarity with the keyboard. He reported that practice in either of these part-processes reacted favorably on the ability to perform the act as a whole. But the fact that he used few subjects and no controls and that the data show large individual variability indicate a

[5] W. H. Norcross, Experiments on the transfer of training, *J. Comp. Psychol.*, 1921, 1, 317–364.

lack of conclusiveness in his results. In Bills and McTeer's work, with conditions well controlled, a positive relation was reported between *logically* identical elements and the amount of transfer developed between alternate tasks. Our approach is a close approximation to the general procedure of Bills and McTeer, except that we have attempted to vary the identity of receptor, effector and central adjustor units of the reaction arc. In respect to these three factors, our results have been negative. The differences between the transfer effects of tasks of varying identity have in no case exceeded their probable errors.

In support of the contention that transfer is not caused by identity of elements in any part of the reaction arc, there may be cited the experiments of Lashley. Animals trained in discrimination habits utilizing one set of receptors or effectors relearned quickly with the use of other pathways. Lashley [6] suggests that if there are common elements, they exist as ratios of excitation in two patterns of integration rather than as identical neural elements. But if this kind of identity is basic to transfer, it should have been effective in the experiments where central pattern was varied.

(Manuscript received May 28, 1937)

[6] K. S. Lashley, Nervous mechanisms in learning in *Handbook of General Experimental Psychology*—Worcester, Clark University Press, 1934, pp. 474–477.

FURTHER STUDY OF THE METHOD OF COMPARISON APPLIED TO THE PROBLEM OF MEMORY CHANGES

BY FRANCIS W. IRWIN AND HERBERT ROVNER

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The method of comparison has been applied to the problem of memory changes by Irwin and Seidenfeld.[1] The results of their experiment demonstrated the existence of definite asymmetries in the distributions of judgments made on characteristics of simple visually perceived figures after intervals of 3 min., 10 min. and 7 days; that is, characteristics of figures identical with those first exposed tended to be over- or under-estimated by groups of *Ss*. The interpretation of these asymmetries met with some difficulties, however, chiefly because of insufficient knowledge of the manner in which the figures were perceived, and the assurance with which the judgments were made. The importance of the *Ss*' perception or *Auffassung* of figures in relation to the kind of memory change to be expected has been stressed by many writers, including Wulf[2] in his pioneer experiment. The *Ss*' assurance of judgment is significant in the method of comparison because it may conceivably affect the amount of asymmetry obtained. The present experiment was performed to acquire knowledge about both the nature of the *Ss*' perceptions and their subjective assurance of judgment.

Method

The method followed closely that of I. & S. A stimulus figure was exposed visually for 5 sec. to a group of *Ss*, who were asked to observe it carefully. After its removal, the *Ss* were told to write as accurate a description of the figure as possible.

[1] Irwin, F. W., and Seidenfeld, M. A., The application of the method of comparison to the problem of memory changes. This *Journal*, 1937, **20**, 363–381. We shall refer to this paper as I. & S.

[2] Wulf, F., Ueber die Veränderung von Vorstellungen (Gedächtnis und Gestalt). *Psychol. Forsch.*, 1922, **1**, 333–373.

When the descriptions of the figure had been written, and about 3 min. after the first exposure, the stimulus figure was shown again, with instructions to the *Ss* to make a judgment on some specified characteristic of it. The *Ss* then made a rating of the assurance of their judgments. This method differed from that of I. & S. in the following particulars:

1. Only one figure was shown to each *S*.

2. Only one judgment was made by each *S*. (The instructions were adapted to these first two changes by substituting the words 'a figure' for 'some figures,' etc.)

3. Additional instructions for description of the figures were given as follows:

"Now I am going to ask you to write on your record sheet a description of the figure I just showed you. Do not make any drawings, but write out as accurate and detailed a description in words as you can. Go ahead."

(Note that when the figures were first exposed the *S* did not know that descriptions would be asked for.)

4. The *Ss* wrote this description before making a judgment.

5. After the judgments had been made the *Ss* were asked to indicate the certainty with which they had made their judgments by writing on the record sheet either 'very certain,' 'fairly certain' or 'pure guess.'

The *Ss* were unfamiliar with the problem and had not served for I. & S. The number of *Ss* was 267, composed of 125 men and 142 women. The records of 6 additional *Ss* were discarded for failure to follow instructions, or because the records could not be understood by the experimenters. The 12 groups of *Ss* ranged in size from 15 to 42 individuals. The *Ss* were students in courses in psychology, most of them being in a first-year course.

Results

The results are presented in Tables I, II and III. In Table I the descriptions of the figures are summarized. The first column names the stimulus figure being described; the second column gives the number of cases who described the figure in the manner indicated in column three; while the last column contains miscellaneous information. We have attempted to indicate in column three what from our point of view were the most important features of the descriptions, and that as far as possible in the *Ss*' own words. A certain amount of subjectivity is inescapable in such a classification; but the use of the *Ss*' words keeps it at a minimum. In a few cases it was difficult to decide whether to go by the words or by the spirit of the description; with these exceptions the task of classification was easy and straightforward.

In Table II are the percentages of judgments in the direction of 'greater' for each judged characteristic (column two), and the number of *Ss* upon which the percentages are based (column three). Columns four, five and six give the per-

TABLE I

SUMMARY OF THE Ss' DESCRIPTIONS OF THE FIGURES

Figure	Number of Cases	Figure Described as:	Remarks
1. Open circle	82 4 3 89	Circle or circular Ring (word 'circle' not used) Arc, hoop, oval (word 'circle' not used)	All Ss mentioned the opening in the figure.
2. Obtuse angle	6 4 1 1 3 3 18	Obtuse angle Angle (more than 90°) Acute angle Angle of 85° Angle of 90° Angle (no statement of size)	All Ss used the word 'angle' in their descriptions. 2 Ss described the figure as part of a diamond. 1 S called it a letter V. The 10 estimates made in degrees: 160°–1; 150°–1; 135°–2; 125°–1; 110°–1; 90°–3; 85°–1.
3. Arc-line figure	12 5 17	Straight line and semicircle Straight line and arc (curvature not specified)	9 Ss mentioned an object: cup, ladel, dipper, wrench, horizontal question mark.
4. Acute angle	12 17 1 1 3 2 5 41	Acute angle Angle (less than 90°) Right angle Angle of 90° Obtuse angle Angle (no statement of size) Word 'angle' not used	4 Ss described the figure as part of a triangle. 2 Ss called it a letter V lying on its side. The 23 estimates made in degrees: 90°–1; 80°–1; 60°–1; 45°–6; 40°–2; 35°–1; 30°–9; 25°–1; 20°–1. 1 S said 'less than 45°'; 1 S said 'less than 90°'.
5. Dot figure	25 1 11 37	Four dots forming imperfect square Quadrilateral (Analytical description, without name of figure)	No object names were used by the Ss. All Ss mentioned the eccentricity of the upper right-hand dot.
6. Open triangle	55 8 1 1 65	Triangle or triangular figure (Analytical description, no figure name) Somewhat like a sailboat Pyramid	4 Ss used object names: billiard triangle, musical triangle (2), sailboat. Of the 55 Ss who called the figure a triangle, 32 called it equilateral, 5 isosceles, 18 did not specify relation of sides.

TABLE II

PERCENTAGES OF 'GREATER' JUDGMENTS AND RATINGS OF ASSURANCE FOR EACH JUDGED CHARACTERISTIC

Judgment Made on:	Percent of 'Greater' Judgs.	Number of Ss	Percent of Ss Rating Assurance as:			Number of Ss
			Very Certain	Fairly Certain	Pure Guess	
Fig. 1—gap of open circle	52	89	19	65	16	68*
Fig. 2—angle of obtuse angle	56	18	22	56	22	18
Fig. 3—curvature of arc of arc-line figure	77	17	31	69	0	16*
Fig. 4—length of short limb of acute angle	28	25	4	76	20	25
Fig. 4—angle of acute angle	69	16	19	63	19	16
Fig. 5—squareness of dot figure	16	37	24	51	24	37
Fig. 6—gap of open triangle	59	65	11	64	25	63*
All *Ss* together		267	17.3	63.0	19.8	243*

* One group of 21 *Ss* did not give assurance ratings. Three individual *Ss* failed to give ratings in the proper form.

TABLE III

PERCENTAGES OF 'GREATER' JUDGMENTS MADE ON EACH CHARACTERISTIC BY THE Ss OF THIS EXPERIMENT AND THAT OF IRWIN AND SEIDENFELD

Characteristic	This Experiment	Irwin and Seidenfeld
1. Gap of open circle	52% (89)	72.5% (167)
2. Angle of obtuse angle	56% (18)	66.5% (167)
3. Curvature of arc of arc-line figure	77% (17)	72.5% (167)
4. Length of short line of acute angle	**28%** (25)	**49.7%** (167)
5. Angle of acute angle	69% (16)	50.9% (167)
6. Squareness of dot figure	**16%** (37)	**46.7%** (167)
7. Gap of open triangle	59% (65)	61.1% (167)

centages of ratings of 'very certain,' 'fairly certain' and 'pure guess,' respectively, with the corresponding number of *Ss* in column seven.

Table III enables the comparison of the percentages of 'greater' judgments obtained in the experiment with those obtained by I. &. S. These percentages are found in columns two and three. Beneath each percentage the number of cases upon which it is calculated is found in parentheses.

Discussion

1. *The Descriptions of the Figures*

The descriptions written by the *S*s have been used as the criterion of the nature of their perception or *Auffassung* of the figures. These reports were written at a psychologically unsophisticated level in practically all instances. They are reports of what the *S*s 'saw'; of 'meaning,' therefore, rather than of 'process.' In no case did we require, or find, a report in terms of sensations and images.

Since we wished to throw light on the results of I. & S. it seemed necessary to use the same instructions before the first exposure of the stimulus figure that they had used. We felt that informing the *S*s in advance that they would have to describe the figure would probably produce an entirely different attitude of observation from that of the former *S*s. They were therefore told, as the others had been: "I am going to show you a figure. I want you to observe it very carefully while it is exposed, so that if I show you a similar figure in the future you will be able to compare it with this one. Remember, look at this figure as long as it is shown, but do not write or draw anything or talk to your neighbor about it."

As stated under *Method*, instructions for the descriptions were read immediately after the 5 seconds exposure of the figure. About 15 seconds were required for the reading of these instructions, after which the *S*s immediately began writing the descriptions. Thus the descriptions were written from memory, and may themselves have been subject to 'memory changes.' It must therefore be kept in mind throughout our discussion that the *S*s have described their memories of their perceptions rather than the perceptions themselves. Although we believe that reports written after so brief an interval would commonly pass for descriptions of perceptions, it would scarcely be appropriate when studying memory changes to overlook this possibility of error. We do not think that the value of the data for our purposes is diminished by this difficulty, since we are interested not so much in the actual perceptions as in the *Auffassungen* of the figures, a term which might be taken to include somewhat

more than perception. At the time of exposure of the dot figure, for example, an *S* might perceive it simply as a visual form on a ground, without verbal elaboration; but when asked to write a description of it he might say to himself, "It looked like an imperfect square," and write accordingly. This whole process, which includes more than mere perception, is undoubtedly important for the problems of later recall, recognition, or comparison, and it is this which, as the *Ss*' *Auffassung*, we are attempting to evaluate. The reader is therefore asked to understand the word 'perception' where it occurs in our discussion more nearly in the sense of the German *Auffassung* than in its own accepted English meaning.

We proceed to discuss the descriptions of each of the six stimulus figures, the results for which have been summarized in Table I.

1. Open circle. The reports on this figure are quite clear, and lead to the conclusion that in the overwhelming majority of cases the *Ss* perceived the figure as an open or incomplete circle. All *Ss* mentioned the opening, most of them using such terms as: 'part missing,' 'break,' 'gap,' 'incomplete,' etc. 84 *Ss* used either the noun 'circle' or the adjective 'circular,' while 4 others called the figure a 'ring.' The remaining 3 *Ss* used the terms 'arc,' 'hoop' and 'oval like the eye.' No other names of objects or figures were given in the descriptions.

2. Obtuse angle. The reports here are more varied in nature. Only 6 of the 20 *Ss* used the term 'obtuse angle,' although 4 more estimated it as containing more than 90 degrees. 3 *Ss* believed it to be an angle of 90 degrees, while 1 *S* said 85 degrees and another called it an acute angle. 2 *Ss* gave no estimate of the size of the angle. 2 *Ss* spoke of the figure as forming part of a diamond, and 1 as a letter V. It seems not to be safe to assume that a large majority of *Ss* will perceive this figure simply as an obtuse angle, even if allowance is made for the possible difficulty our *Ss* had in expressing in degrees the angle they had perceived.

3. Arc-line figure. Here the *Ss* uniformly perceived the straight line as such. 12 *Ss* perceived the arc as a semi-

circle; the remaining 5 *Ss* called it an arc or curve. More than half of the group (9 of 17) used object names in their descriptions, such as 'dipper,' 'cup,' 'question mark lying on its side,' etc. With this figure, then, one cannot count upon all *Ss* perceiving the arc as a semicircle; and of the six figures used, this one seems most likely to be perceived as, or to cause the *Ss* to think of, an object.

Of the 10 *Ss* who mentioned the relative lengths of the straight line and the diameter of the arc, the line was reported longer than the diameter by 1 *S*, less than the diameter by 1 *S*, and equal or approximately equal to the diameter by 8 *Ss*. The straight line actually measured 54.5 mm. and the diameter 50 mm.

4. Acute angle. This figure seems to have caused more difficulty in description than any of the others. 29 of the 41 *Ss* either used the term 'acute angle' or estimated the angle at less than 90 degrees. 2 called it a right angle, and 3 an obtuse angle. The difficulties experienced by some of the *Ss* in making estimates in degrees, and their unfamiliarity with geometrical terms, may be suggested by the fact that 1 *S* said the figure was 'not quite a right angle—about 45 degrees,' while another said it was 'an obtuse angle of about 35 degrees.'

17 of the 41 *Ss* reported the base to be shorter than the slanting limb, but one said the base was the longer, and 2 said the limbs were about equal. 1 *S* said they were unequal without specifying which was the longer. The other 20 *Ss* made no mention of the length of the lines.

Few *Ss* named objects or figures other than an angle. 3 spoke of it as a triangle with a missing side. 1 mentioned the letter V. No other object or figure names were given.

For this figure, as for the obtuse angle, the reports show considerable heterogeneity. How much of this is to be attributed to perception, and how much to difficulties of description, is uncertain; but it is not safe to assume general homogeneity of perception for either figure. The influence of associated objects and figures appears to be unimportant in the case of the acute angle.

5. Dot figure. The descriptions indicate clearly that this figure was perceived by 25 of the 37 *Ss* as a figure departing

somewhat from a square. The *S*s frequently said it 'resembled a square,' or that it would have been a square except that one dot was 'out of line,' etc.

For 11 of the *S*s, however, there is no evidence that the figure was perceived as a square. These *S*s gave analytic descriptions in terms of lines, angles and distances. 1 additional *S* used the word 'quadrilateral.'

No *S*s used names of objects, or of figures other than those mentioned here.

6. Open triangle. This figure can be said to have been perceived as a defective triangle or triangular figure by the great majority of *S*s. 55 of the *S*s used one or the other of these terms. All *S*s observed the gap; the reports were: 'apex cut off,' 'upper end open,' 'not joined at top,' 'sides not meeting,' etc.

Of these 55 *S*s, 32 specified 'equilateral' and 5 'isosceles.' The remaining 18 gave no report on the relative length of the sides.

10 *S*s gave analytical descriptions without using the term 'triangle.' The following object- and figure-names appeared: musical triangle (2 *S*s), billiard triangle, sailboat, pyramid.

2. *Conclusions from the Descriptions, and Applications to the Results of Irwin and Seidenfeld*

*S*s tend strongly to perceive the open circle and open triangle as an incomplete circle and incomplete triangle. The tendency to perceive the dot figure as an imperfect square is somewhat less strong. Very little reference to objects or other figures is made in the case of these three figures.

The arc-line figure is perceived by many *S*s as an object of some sort. The straight line is perceived as straight; the arc tends to be perceived as a semicircle with a diameter about equal to the length of the straight line.

*S*s vary considerably in their descriptions of the acute and obtuse angles. The acute angle was seen by a small majority of *S*s as acute, or as containing less than 90°. Half of the *S*s saw the obtuse angle as obtuse, or containing more than 90°. Apparent unfamiliarity of the *S*s with estimation of angles in degrees, and with geometrical terms, makes it difficult to

determine in many instances how these two figures were perceived. It seems fair to conclude that there is more real variety of perception in the case of these two figures than of any of the other four figures used in this experiment.

These results strengthen the possibility that the closing of the gaps in the open circle and open triangle, and the increased squarenesss of the dot figure, which are indicated in the memorial processes by the results of I. & S., are to be explained by the laws of *Prägnanz* and closure. The gaps seem to be perceived as gaps, and the dot figure as an imperfect square, in enough cases to permit the prediction from these laws of asymmetries in the obtained directions.

There are no indications that the obtuse and acute angles were perceived as 'imperfect right angles'; hence, the memorial narrowing of the obtuse angle and widening of the acute angle can probably not be interpreted, as had been suggested by I. & S., as an approach of each to the 'standard figure' of a right angle. The descriptions of these two figures, taken as a whole, are too ambiguous to make possible a definite explanation of the observed asymmetries of judgments on the angles.

A large majority of those of our *Ss* who mentioned the relative lengths of the limbs of the acute angle reported correctly that the base was shorter than the upper limb. However, only about one-half of the *Ss* reported on this at all. We are not ready to interpret the tendency for the short line to become shorter in memory, as found by I. & S., particularly when this tendency did not appear in their results for the 3 min. interval.

Nothing in the *Ss*' reports helps us to understand the decreased area of the open circle and the increased area of the dot figure and the open triangle.

No highly probable explanation of I. & S.'s finding of a decreased curvature of the arc of the arc-line figure is suggested by the present results. The frequent use of object names may be of importance; but the objects mentioned by the *Ss* are too varied in nature to permit prediction of the direction of change to be expected by object-assimilation or Wulf's '*Normalisierung*.' There is also a memorial decrease

in the length of the straight line of this figure, which may be considered together with the fact that the line, although physically a little longer than the diameter of the semicircle (54.5 mm as against 50 mm), tended to be perceived as about equal to it in length. We suggest that an *S* who originally perceived the two lengths to be equal, or approximately so, would be likely to judge the present length of the straight line to be greater than the original because, when forced to make a judgment of greater or less, he would tend to observe the characteristic in question more analytically; hence he would be more likely to perceive that the straight line he is now observing is longer than the diameter of the semicircle. This would be true whether his original perception of approximate equality was due to lack of sufficiently careful observation, or was an illusion. (If it were the latter, our argument depends upon the generally accepted principle that the magnitude of an illusion is diminished with more analytical observation.) This explanation of the decrease of length of the straight line could be phrased in the terms either of Gestalt or associationism; we do not see how a decision between the two kinds of theory can be reached in this particular case. The influence of object-reference may, of course, be present; but it cannot be evaluated from the data at hand.

3. *Comparison of the Asymmetries of Judgment with those of Irwin and Seidenfeld*

Although the determination of asymmetries of judgment was not a part of the purpose of this experiment, it may be of interest to compare the judgment asymmetries with those of the previous experiment. Table III gives the percentages of 'greater' judgments on each of the seven characteristics judged upon in this experiment. The first column names the characteristic judged; the second column gives the percentage of 'greater' judgments obtained in the present experiment, with the number of *S*s in parentheses beneath the percentage; the third column gives similar data for the 3 min. interval from the experiment of I. & S. Percentages below 50 percent are printed in bold-face type.

Two things are apparent in this comparison. First, the

differences between the percentages obtained in the two experiments are in general large. Second, the directions of asymmetry (deviations of the obtained percentages from 50 percent) are in all seven cases the same in the two sets of results.

The large differences in percentage may, we believe, be accounted for by the small number of *Ss* who judged most of the characteristics in the present experiment, and possibly also by the description of the figure which each *S* wrote before making his judgment. The fact that the directions of asymmetry are identical in the two sets of values is encouraging, particularly since by chance alone this would occur in only one case out of $\frac{1}{2}^7$, or once in 128 times.

We had hoped to be able to present data to show the presence or absence of effects of the mode of perception upon the amount or direction of the asymmetries of judgment. This did not prove feasible, however, primarily because the large number of subclasses into which the *Ss*' perceptions had to be divided rendered the number of cases in each subclass too small for statistical treatment.

4. *Subjective Assurance of Judgment*

The amount of guessing done by *Ss* in forming their judgments in the experiment of I. & S. was not known. Since the method of equal stimuli was used, the possibility that a large proportion of the judgments amounted simply to guesses had to be considered. The larger the proportion of judgments that were pure guesses, the smaller would be the expected asymmetries, since such judgments by chance should fall about equally into the two categories used in the case of each characteristic.

The results given in Table II show that on the whole less than 20 percent of the judgments were rated 'pure guesses,' while almost 20 percent were 'very certain.' A majority of the ratings, 63 percent, fell into the middle category, 'fairly certain.' The maximum proportion of 'pure guesses' for any one characteristic was 25 percent (gap of open triangle); the minimum was 0 percent (curvature of the arc of the arc-line figure).

It might be supposed that *S*s who made the same judgment on a figure as the majority of the group made would have a higher degree of assurance on the average than those who judged with the minority, on the grounds that the amount of 'memory change' for the former group would be likely to be greater than for the latter. Taking together the results for all seven characteristics, the percentages of 'very certain,' 'fairly certain' and 'pure guess' ratings, respectively, were as follows:

*S*s judging with the majority: 18.5%, 60.9%, 20.5%.
*S*s judging with the minority: 15.2%, 66.3%, 18.5%.

The supposition is thus shown to have little basis in fact. *S*s who judge with the majority have a slightly greater percentage of 'very certain' judgments, but also a slightly greater percentage of 'pure guess' judgments. Whether this means that we are wrong in supposing that greater amounts of memory change are associated with larger proportions of cases making the same judgment, we cannot say. The assumption seems reasonable from general psychophysical principles. The assurance of judgments cannot, of course, be taken as a reliable indication either of their correctness or of the *S*'s sensitivity.

We conclude that, so far as subjective ratings can be used as a criterion, the method of equal stimuli as used in this experiment does not appear to have produced an amount of guessing greater than that found in the usual psychophysical procedures. The same conclusion can, we believe, be applied to the experiment of I. & S. because of its close correspondence in method.

Summary

This experiment was performed to gain additional information on certain questions left unsettled in a previous experiment by Irwin and Seidenfeld. Descriptions by the *S*s of the visually perceived stimulus figures were obtained in order to infer the nature of the *S*s' perceptions of these figures. Data on asymmetries of judgment and on subjective assurance of judgment were also obtained.

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OCULAR DOMINANCE AND THE RANGE OF VISUAL APPREHENSION *

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I. Introduction

The problem of eyedness forms an integral part of the larger topic of the functional, bilateral asymmetry of the human organism. For the determination of the dominant eye, numerous techniques have been devised which give more or less inconsistent results. The assumption basic to the various tests, however, is that eyedness is an expression of a whole eye. As is well known, the neural connections from each retina are divided between the two hemispheres. Therefore, if, as has widely been supposed, unilateral tendencies depend upon the dominance of one hemisphere over the other, it would seem that some correspondence in function might be found between the corresponding halves of the two eyes. Differences might also occur in the accuracy and amount of visual apprehension between the nasal and temporal halves of the same eye. Accordingly, the object of the present investigation may be stated briefly as follows: the determination of the relationship between ocular dominance as measured by tests employing the whole eye, and the relative amount and accuracy of visual apprehension in the four visual fields, using monocular, tachistoscopical presentation of stimulus material.

A number of investigations have been made on the relation between lateral dominance and position of the stimulus in the report of visually perceived material. Stevens (12), in 1908, presented subjects with white cardboard discs at different points in the field of vision. He observed a tendency for right-handed subjects to over-estimate objects in the right

* The author wishes to acknowledge indebtedness to Dr. L. F. Beck for his assistance and guidance in this study.

field and for left-handed subjects to over-estimate objects in the left field. In 1912, Stevens and Ducasse (13) published the results of an investigation in which subjects were asked to compare variable with standard extents in the different fields of vision. It was found that the right halves of the horizontal and right oblique meridians were consistently over-estimated. Both eyes agreed as to the direction and amount of over-estimation. Stevens and Ducasse concluded that corresponding halves of the retina have an identical space sense with respect to the kind and magnitude of constant space error. They also compared the proportion of right-handed individuals with the tendency to over-estimate objects on the right and suggested that both of these unilateral tendencies resulted from the dominance of the left cerebral hemisphere.

Dallenbach (5), on the other hand, found that circular areas to the left and above the point of fixation required less illumination to equal a standard than those below and to the right. In a later study (6) he again observed that the left position had an attentional advantage and noted that his three subjects were right handed.

More recently, Anderson and Crosland (1) have investigated the effects of eye dominance on 'range of attention' scores, using binocular, tachistoscopical presentation of material. Their subjects were divided equally with respect to eyedness, handedness, and sex. They found the left-eyed subject to excel the right-eyed subject at the right side of an exposure card of nine letters; the right-eyed subject to be superior to the left-eyed subject for the left field letters. Anderson and Crosland (2) also studied the effects of combinations of handedness and eyedness on 'range of attention' scores. They found that under conditions of left print (letters printed on the left half of the card) very small differences occurred. Under conditions of center print and right print (letters printed on the right half of the card), left-handed, left-eyed subjects excelled and right-handed, right-eyed subjects made lowest scores. Right-handed, left-eyed, and left-handed, right-eyed subjects fell in the middle ground.

II. Procedure

A modification of the Dodge mirror tachistoscope (7) was used in presenting the stimulus material for measuring the span of visual apprehension. The two illuminated fields were approximately three and one-half inches high and five inches wide. The cards which were inserted in the exposure field contained a row of nine letters each, spaced approximately one-half inch apart and arranged so that the center letter fell at the point occupied by the cross on the pre-exposure field. No two cards were alike, and the combinations of letters within each card in no case formed a familiar word or syllable. The eye piece on the tachistoscope was adjustable so that it might be used for either eye. It was shielded on four sides by black cardboard so that extraneous visual cues for the eye not in use were eliminated.

The stimulus material was presented for an interval of 120 milli-seconds in order to rule out the possibility of more than one fixation during the interval. The necessity of maintaining fixation on the center of the cross was emphasized. As a further check against the tendency to read from left to right, the subjects were instructed to record letters in three different directions: (1) left to right, (2) right to left, and (3) center-alternate, that is, first the center letter, next the first to the left, then first to the right, etc. The order of recording to be used was varied in random order from trial to trial and was not known until the time for recording. A total of 48 cards was presented, 24 to each eye in groups of 12 each.

In determining ocular dominance, the following tests were used: 1. stereoscope test, 2. Parson's manoptoscope (10), 3. Scheidemann's paper-sighting test (11), 4. Mill's convergence test (9), and 5. a modification of the traditional cancellation test which aimed to detect possible differences between the two eyes in the ability to select items rapidly.

Fifty men, all university students between the ages of 18 and 24, acted as subjects for the experiment. Forty-six of the men were right-handed, four were left-handed. Handedness was determined by performance on a battery of tests of manual skill compiled by L. F. Beck (3). Those subjects who wore glasses wore them also during the experiments.

III. Results

Determination of Ocular Dominance.—There was a marked lack of agreement found between the results of the five tests of ocular dominance administered. The percentage of agreement between tests in no case exceeded 68 percent. The cancellation test, scored for both time and errors and the stereoscope test showed less than 50 percent agreement with the results of all other tests. Only three tests showed 60 percent or more agreement with each other. They were the manoptoscope, paper-sighting and convergence tests. Crider (4) has reported similar observations. In analyzing 18 studies of ocular dominance, he found a wide variation in the percentages of right, left, and impartial eyedness reported. He also demonstrated that the percentage of impartial eyed-

ness increases with the number of sighting opportunities allowed.

It was arbitrarily decided that in this investigation a subject would be considered to prefer that eye which he used consistently on two of the three most reliable tests (paper-sighting, manoptoscope, and convergence). With this criterion, it was found that there were 30 right-eyed and 16 left-eyed subjects. Results on the remaining four subjects were doubtful.

Range of Visual Apprehension.—The record sheets for the range of visual apprehension test were graded in two different ways. First, the number of letters reported correctly in their correct positions was tabulated for each subject. Next, the number of letters reported which were on the stimulus cards, regardless of whether or not the position was correct, was determined. In order to rule out possible practice effects, analysis was confined to the results of the last two trials.

When the results were pooled, it was found that the total number of letters recalled was greatest at the center position, next greatest at the two positions on either side of the center letter, and least in the three positions furthest from the center on either side. It would seem therefore that fixation was well maintained throughout the experiment.

A comparison between the average number of letters recalled for each eye revealed that there was no significant difference between the right and left-eyed subjects with respect to the scores obtained by either eye. That is, the left-eyed subjects recalled as many of the letters which were presented to the right eye as did the right-eyed subjects, and vice-versa.

Similarly, a comparison between the right and left-eyed subjects with respect to the recall of letters presented to the four retinal halves of the two eyes (right and left nasal, and right and left temporal) showed no differences that were statistically significant. In other words, it was impossible to differentiate between the right and left-eyed subjects on the basis of relative amount of recall from either the right or left sides of the stimulus card.

These results do not seem in accord with those obtained by Anderson and Crosland (1), Dallenbach (5, 6) and others. Although two tests of the four used by Anderson and Crosland in determining the ocular dominance of their subjects were also used as a basis of classification in this experiment, it is possible that the disparity between results is due to differences in the criteria by which the subjects were classified. The fact that monocular presentation was substituted for binocular presentation in this experiment may also have had some effect. The visual angle subtended by the distance between the first and last letters was much larger in our stimulus material than in that used by Anderson and Crosland. This would cause a greater number of letters to fall outside the macula in our experiment. Bilateral asymmetry may not be uniform within and without the macular region.

Tables I and II give scores for the span of visual apprehension in the two halves of each eye for both groups.

TABLE I

COMPARISON OF RECALL OF LETTERS WHICH FALL ON THE NASAL AND TEMPORAL HALVES OF THE RETINA

Group	Eye	Field	Total	Mean	σ	$\frac{\text{Diff.}}{\sigma \text{ Diff.}}$	Chances in 100 of a True Diff.
Right-eyed	Right	Nasal	424	14.1	3.87	1.7	96
		Temporal	356	12.2	5.03		
	Left	Nasal	400	13.3	3.82	1.22	88
		Temporal	436	14.5	3.76		
Left-eyed	Right	Nasal	227	14.2	2.96	1.87	97
		Temporal	187	11.7	4.46		
	Left	Nasal	205	12.8	4.41	1.59	94
		Temporal	243	15.2	4.14		

An inspection of the total number of letters recalled for the nasal and temporal fields of each eye shows that, with one exception, the scores for the nasal side of the right eye exceed those for the temporal side, and the scores for the tem-

TABLE II

COMPARISON OF RECALL IN CORRECT POSITION OF LETTERS WHICH FALL ON THE NASAL AND TEMPORAL HALVES OF THE RETINA

Group	Eye	Field	Total	Mean	σ	$\frac{\text{Diff.}}{\sigma \text{ Diff.}}$	Chances in 100 of a True Diff.
Right-eyed	Right	Nasal	249	8.3	3.6	0.46	67
		Temporal	237	7.9	3.6		
	Left	Nasal	296	9.9	3.4	0.75	77
		Temporal	277	9.2	3.2		
Left-eyed	Right	Nasal	136	8.5	2.4	1.1	86
		Temporal	116	7.25	3.8		
	Left	Nasal	156	9.75	4.3	0.4	65
		Temporal	167	10.4	3.2		

poral side of the left eye exceed those for the nasal side. In other words, although the differences between the means are small there seems to be a tendency for letters on the right side of the fixation point to be recalled more efficiently than those on the left side. This holds true for either eye and for both right-eyed and left-eyed subjects. This observation tends to agree with the findings of Stevens and Ducasse (**13**) and is what we might expect from the neural connections of the eye as far as the subjects with right lateral dominance are concerned. It is more difficult to explain why the left-eyed subjects also tended to recall more from the right side of the stimulus card. It must be remembered that all but two of these left-eyed subjects were right-handed. These subjects also showed greater variability on the tests for ocular dominance administered. The group, therefore, was not homogeneous and probably contained a number of individuals with stronger right lateral dominance than left. In any case, ocular dominance as here measured, showed no relation to the relative amount of recall from either the right or left visual fields. This tends to support the results of Raney and Jasper (**8**) who found little or no relation between ocular dominance as measured by tests of unilateral sighting and dominance as measured by the Phi test.

The coefficients of correlation (product moment) which express the relation between the functioning of the two halves of the same eye and corresponding halves of the two eyes are given in Tables III and IV.

TABLE III

RELATION BETWEEN VISUAL APPREHENSION IN THE TWO HALVES OF THE SAME EYE AND BETWEEN CORRESPONDING HALVES OF THE TWO EYES
(RIGHT-EYED GROUP)

	Recalled in Position		Irrespective of Position	
	R. Nasal L. Temporal	R. Temporal L. Nasal	R. Nasal L. Temporal	R. Temporal L. Nasal
r.	0.71	0.79	0.77	0.9
	R. Nasal R. Temporal	L. Nasal L. Temporal	R. Nasal R. Temporal	L. Nasal L. Temporal
r.	0.31	0.22	0.29	0.36

TABLE IV

LEFT-EYED GROUP

	Recalled in Position		Irrespective of Position	
	R. Nasal L. Temporal	R. Temporal L. Nasal	R. Nasal L. Temporal	R. Temporal L. Nasal
r.	−0.16	0.57	0.19	0.41
	R. Nasal R. Temporal	L. Nasal L. Temporal	R. Nasal R. Temporal	L. Nasal L. Temporal
r.	0.20	0.09	0.36	0.24

The coefficients between the amount of visual apprehension in the two halves of the same eye are low, sufficiently low to permit the statement that there is little or no correspondence between them. In short, it is impossible to predict the recall of letters in the nasal half of one eye from a knowledge of the recall in the temporal half of the same eye or vice-versa.

With respect to the relation between the function of corresponding halves of the two eyes, the coefficients show a

positive relationship. For the right-eyed group, these coefficients are consistently high, falling between 0.7 and 0.9. It would seem then, that for the right-eyed group there is greater concomitant variation in function between corresponding halves of the two eyes than between the two halves of the same eye. This might be expected from our knowledge of the neural connections of the eye. For the left-eyed group, however, the coefficients are much lower, even statistically not significant in all cases but one. It is interesting to note that the only significant coefficient among the scores of this left-eyed group was between the visual apprehension in the left nasal and right temporal halves, corresponding halves whose neural connections terminate in the right cerebral hemisphere.

IV. Summary and Conclusions

In this investigation an attempt was made to determine the effect of ocular dominance on the relative recall of letters falling on the right and left retinal halves of the two eyes. Fifty undergraduate men acted as subjects for the experiment. Forty-six of these men were right-handed, four were left-handed. In determining ocular dominance, the following tests were used: 1. manoptoscope, 2. stereoscope, 3. paper-sighting, 4. convergence, and 5. cancellation. Monocular, tachistoscopic presentation of material was used to determine the range of visual apprehension for stimuli falling in the right and left halves of the retinae in each eye.

It was found that: 1. There was a marked lack of agreement between the results of the tests of eyedness administered. This indicates that the reliability of such tests may be questioned. 2. Ocular dominance, as here measured, had no effect on: (*a*) the range of visual apprehension in either eye and (*b*) the relative amount of recall of letters on either side of the stimulus card. 3. Both right and left-eyed subjects showed a slight tendency to recall more letters in the right visual field than in the left. Since all but four of these subjects were right-handed, this suggests that there may be some relationship between handedness and the relative amount of recall in the right and left visual fields. No de-

finite conclusions may be drawn on this point without further experimentation. 4. Coefficients of correlation expressing the degree of functional relationship between the two halves of the same eye were not significant. However, when the corresponding halves of the two eyes were compared, the coefficients proved to be consistently high (0.7 to 0.9) for the right-handed, right-eyed group. Thus, for this group at least, there is a functional relationship between retinal halves whose neural connections terminate in the same hemisphere.

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References

1. Anderson, I., and Crosland, H. R., The effects of eye dominance on 'range of attention' scores, *U. of Oregon Publ.*, 1933, **4**, No. 4, pp. 24.
2. ——, The effects of combinations of handedness and eyedness on letter-position, 'range of attention' scores, *U. of Oregon Publ.*, 1934, **4**, No. 7, pp. 48.
3. Beck, L. F., Manual skills and the measurement of handedness, *J. Psychol.*, 1936, **2**, 259–272.
4. Crider, B., Unilateral sighting preference, *Child Develop.*, 1935, **6**, 163–164.
5. Dallenbach, K. M., Position *vs.* intensity as a determinant of clearness, *Amer. J. Psychol.*, 1923, **34**, 283–286.
6. —— and Dewey, D., Size *vs.* intensity as a determinant of attention, *Amer. J. Psychol.*, 1924, **35**, 121–125.
7. Dodge, R., An improved exposure apparatus, *J. Applied Psychol.*, 1907, **4**, 10–13.
8. Jasper, H. H., and Raney, E. T., The Phi test of lateral dominance, *Amer. J. Psychol.*, 1937, **49**, 450–457.
9. Mills, L., Eyedness and handedness, *Amer. J. Ophthol.*, 1925, **8**, 1–8.
10. Parson, B. S., *Left-handedness*, Macmillan & Co., N. Y., 1924.
11. Scheidemann, N. V., A simple test of ocular dominance, *Amer. J. Psychol.*, 1931, **43**, 126.
12. Stevens, H. C., Peculiarities of peripheral vision, *Psychol. Rev.*, 1908, **15**, 69–93.
13. —— and Ducasse, C. J., The retina and right-handedness, *Psychol. Rev.*, 1912, **19**, 1–31.

THE OPTIMAL LOCUS OF 'ANTICIPATORY TENSIONS' IN MUSCULAR WORK*

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Whether muscular tension will improve or hinder a given performance depends upon a number of factors including (1) the *amount* of tension developed, (2) the *timing* of the proprioceptive impulses in relation to impulses of the primary reaction pattern and (3) the *neuro-anatomical* connections which exist between the tonically contracting muscles and those involved in the test performance. That these factors are mutually interdependent has already been suspected by certain investigators, including the writer.[1] This paper reports the effect of changing the anatomical proximity of anticipatory tensions relative to the muscular pattern involved in finger oscillation.

Before describing the experiments in detail, it is well to say what the writer means by 'anticipatory tension.' This state of more or less steadily maintained muscle contraction (voluntarily or involuntarily induced) should not be confused with 'tonus' in the strictly physiological sense of a 'stretch reflex,' though the two are provably related.[2] Tonic contraction, such as can be initiated by the verbal command, "tense your arm," is here distinguished from the phasic contraction typical of such muscular work as calisthenics and

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[1] *Cf.* H. Peak, Modifications of the eyelid reflex by voluntary induced sets, *Psychol. Monog.*, 1931, **42**, 1–68. G. L. Freeman, The facilitative and inhibitory effects of muscular tension upon performance, *Amer. J. Psychol.*, 1933, **45**, 17–52.

[2] When a muscle contracts tonically, it stretches its agonist. Proprioceptors are stimulated in both muscles, and the impulses therefrom accrue to the neural frux due to already existing slight stretch or tonus of the muscles. Impulses due to stretch and those produced by tonic contraction seem to have different centripetal effects, but the underlying neurological assumptions are vague.

tapping. A given muscle may be in a state of tonic contraction and at the same time contract phasically in response to appropriate stimulation; or tonic contraction may be induced in one muscle group while phasic contraction is centered in another muscle. When tonic contraction is induced immediately prior to beginning a phasic muscular task, we have a state of 'anticipatory tension.' It is an observable phenomena which continues in some degree during the test performance, and which is subject to definite experimental manipulation and control. Elsewhere [3] the writer has identified this term with neuromuscular 'spreads,' 'sets,' and 'postures.' Preference for the phrase, 'anticipatory tension,' is advanced on the following grounds: (1) the phenomena is definitely related to those neuromuscular conditions which are antecedent to and anticipatory of the majority of our voluntary activities; (2) most tension which facilitates is that which is initiated in anticipation of the test performance; and (3) the most feasible way to study effects of tension involves the induction of such contractions prior to the beginning of the phasic muscular activity.[4]

Previous work has indicated that under certain conditions one may expect maximum performance when anticipatory tension is centered or proximate to muscles engaged in the task. Thus Peak [5] found that the amplitude of the eye-lid is increased when the subject is 'set' to make a voluntary lid closure; and Freeman [6] showed that finger oscillations are more rapid when the ipsilateral rather than contralateral arm extensors sustain a weight.

On the other hand, the failure of certain investigators [7] to

[3] G. L. Freeman, *Introduction to Physiological Psychology*, New York, Ronald Press, 1934, p. 433 ff.

[4] Fuller discussions of the role of 'anticipatory tension' in performance are found in J. F. Dashell, *The Fundamentals of Objective Psychology*, 1928, p. 275 ff., and G. L. Freeman, *op. cit.*

[5] H. Peak, *op. cit.*

[6] G. L. Freeman, The facilitative and inhibitory effects of muscular tension upon performance, *Amer. J. Psychol.*, 1933, **45**, 45 ff.

[7] *Cf.* F. L. Golla and S. Antonovitch, The relation of muscular tonus and the patellar reflex to mental work, *J. Ment. Sci.*, 1929, **75**, 234–241, and E. H. Henley, Factors related to muscular tension, *Arch. Psychol.*, 1935, no. 183, p. 44.

obtain facilitation with tension variously localized suggests that the relation is more indirect and subject to subtle changes. Specifically, different degrees of tonic contraction in a given muscle may have differential effects upon performance involving a related muscle group. In a preliminary test of this possibility, thirty undergraduate students engaged in rapid oscillation of the middle finger while the effect of various tensions in the right and left arm flexors was systematically explored. Oscillation of the middle finger was chosen as a test performance because this member can be operated more or less independently of the other digits. With amplitude of oscillation controlled by use of appropriate stops, rate becomes the only variable in the performance.

Subjects sat in a comfortable chair with elbows firmly supported and with hands inserted into gloves having the middle finger cut out. The remaining fingers of these gloves were tacked to a form which held the wrist arched upward and which permitted movement of only the middle finger. Directly over the wrists of each hand were located spring postal scales, inverted so that pressing upward against the platform would place the arm flexors under measurable tension (see Fig. 1). In experiments where it was also desired to place the arm extensors under tension, equivalent weights acted over pulleys against the subjects' wrists, a counterpoise being inserted into the system to compensate for the dead weight of the lower arm. Finger oscillations were recorded by means of a smaller postal scale connected with an electro-magnetic counter, and running on a tract so that its inverted platform could be connected with the middle finger of either right or left hand. A black cloth hid the platforms from the subject's view, but the scale-faces were turned away so as to be seen over the apparatus top by means of mirrors.

The general instructions were: "Do you feel the large platform over your right wrist? Push up against it until the pointer on the scale reaches the red mark. Hold it there. Now the tip of your middle finger is attached to a small platform. Push up with your middle finger so that the pointer on the smaller scale just reaches the red mark. Now push your finger down until it strikes the stop. Oscillate the finger back and forth this way as rapidly as possible, always remembering to push the small scale pointer to the red mark, and also to maintain your wrist pressure."

After the subject was thoroughly familiar with the procedure, twenty-five thirty-second periods of finger oscillations were recorded for the right and middle finger under the following conditions, rotated to avoid practice effects.

A. No artificially induced tension.
*B*1. ½ lb. artificially induced tension in right flexors.
*B*2. ½ lb. artificially induced tension in left flexors.
*C*1. 1½ lb. artificially induced tension in right flexors.
*C*2. 1½ lb. artificially induced tension in left flexors.

A one minute rest was taken between conditions and a five minute rest between the five cycles.

The average thirty-second scores in finger oscillations for the group of thirty subjects were:

Control (*A*): 110.

½ lb. tension (*B*): ipsilateral side, 116; contralateral side, 106; combined, 111.

1½ lb. tension (*C*): ipsilateral side, 120.5; contralateral side, 126.8; combined, 123.7.

It is apparent that the 1½ lb. tensions were the most effective for the majority of subjects; ½ lb. tensions produced no increment over the control state. There is a slight suggestion that the ½ lb. tensions were more effective when induced in the ipsilateral side, and the 1½ lb. tensions more effective when induced on the contralateral side. But the variability of these scores indicated a high probable error.

There are at least two different explanations for the ambiguity of these results: (1) the subjects might have been unconsciously exerting tensions in other muscles during any or all of the tests, so that the artificially induced tension of the moment was cancelled or augmented by unequated tensions elsewhere; or, (2) differences might have been confused or obliterated by uncontrolled central factors, such as holding the attitude that it is easier to maintain tension in one hand than another, or shifts in attention occasioned by the double instruction to tap and maintain pressure. The first supposition was proved unlikely by galvanic recording made from the palm of several subjects during the tests. These readings generally showed regular increments under increased weight-loads, which would mean that the artificial tension was reflected in the general reactivity level of the organism.[8] We accordingly turned our attention to uncontrolled central factors as the more important.

When it is simply reported that artificially induced tension did or did not cause improvement in a test performance, we

[8] Accumulating evidence strongly suggests that palmar galvanic conductance reflects tension changes. *Cf.* C. W. Darrow and G. L. Freeman, Palmar skin resistance changes contrasted with non-palmar changes, and rate of insensible weight loss, *J. Exp. Psychol.*, 1934, 17, 739–48; M. A. Wenger and O. C. Irwin, Fluctuations in skin resistance of infants and adults and their relation to muscular processes, *Univ. of Iowa Studies.*, 1936, 12, 143–206; G. L. Freeman and R. M. Simpson, The effect of experimentally induced tension upon palmar skin resistance (in press).

do know whether the result is due to inherent neurological connections, or is mediated by the more variable higher arcs under the influence of 'self instruction.' For this reason our next attack was centered on a comparison of tension effects under controlled and uncontrolled suggestion. Four subjects took part in this experiment. Oscillations of the right middle finger were recorded over thirty second periods, with a 1½ lb. tension [9] induced in the right or left arm flexors. Conditions were properly counterbalanced to minimize practice effects, with five minutes rest between cycles. In the first series, the subjects were told only that "we want to see how rapidly you can oscillate your middle finger when different muscles sustain weight," thereby leaving room for uncontrolled self-instruction. During an equivalent number of sessions, subjects operated under instructions devised to control and give direction to suggestion effects. When tension was induced on the same side of the body as the finger oscillation, the experimenter said, "Now you can just push up the platform with the same hand that is doing the tapping." When tension was induced on the contralateral side, the experimenter said, "Now you will have to divide your attention between your two hands while you work."

As shown in Table I, the effect of the definite suggestion was positive. Performance improved in the presumptively easier task of maintaining tension while tapping in the same hand. The records obtained with uncontrolled instructions showed no such general agreement, different subjects doing better with tension in different hands. The subjects' commentaries indicate that during the experiment they talked much about which task was the easier. Practically all reported that one task was *assumed* easier than another. Because there is little significance in the work of individual cycles, the data are presented in Table I as the average number of finger oscillations per thirty seconds under conditions *A* (tension in ipsilateral flexors) and *B* (tension in contralateral flexors).

[9] Previous work with ½ lb., 1½ lb. and 3 lb. weights had generally shown the 1½ lb. weight to be the most effective in facilitating performance.

TABLE I

THE EFFECT OF CONTROLLED AND UNCONTROLLED SUGGESTION UPON RATE OF FINGER OSCILLATION WITH 'ANTICIPATORY TENSIONS' INDUCED IN IPSILATERAL (*A*) AND CONTRALATERAL (*B*) ARM FLEXORS

Subject	Av. Oscil. per 30 Sec. under Uncontrolled Suggestion		Av. Oscil. per 30 Sec. under Suggestion that *B* Condition Was more Difficult	
	Tension Conditions		Tension Conditions	
	A	*B*	*A*	*B*
E	92.5	94.0	96.4	90.0
T	150.0	134.4	145.5	130.0
S	139.5	139.3	135.0	116.0
B	123.0	128.5	112.3	98.0
Av.	126.3	124.1	122.3	108.5

Equally significant were the results obtained from two other subjects; here the suggestion that the finger would be oscillated more rapidly with tension localized in the opposite hand resulted in an average gain of 15 points.

It seems apparent that the results of many experiments with apparently induced tension have been complicated by the presence of uncontrolled central factors such as varying self-instruction or attentional shifts in a bifurcate task of maintaining tonic contraction in one muscle group while engaging in phasic activity with other parts of the neuromuscular apparatus. The next experiment sought to divorce these central factors from the equation by long periods of practice in which the maintenance of tonic contraction became automatic. Instructions were to concentrate on the test performance, with the experimenter warning of any failure to maintain tension. The same apparatus was employed as before, with oscillations of the right middle finger during counterbalanced rotation of the following conditions:

A. 1½ lb. tension induced by ipsilateral arm flexors.
B. 1½ lb. tension induced by ipsilateral arm extensors.
C. 1½ lb. tension induced by contralateral arm flexors.
D. 1½ lb. tension induced by contralateral arm extensors.

Results for the six subjects are given in Table II.

TABLE II

THE EFFECT OF 'AUTOMATICALLY MAINTAINED' TENSION IN RELATED MUSCLE GROUPS UPON THIRTY-SECOND RATE OF FINGER OSCILLATION

Subject	Ipsilateral Flexors (*A*)	Ipsilateral Extensors (*B*)	Contralateral Flexors (*C*)	Contralateral Extensors (*D*)
L	187.5	181.5	166.5	187.5
P	178.2	167.0	162.5	166.2
T	157.7	161.5	153.2	156.5
M	154.5	150.0	149.0	144.0
V	147.0	143.2	127.5	135.0
R	157.5	150.7	146.0	139.0
Av.	163.7	159.0	150.8	154.7

The table shows that under these special conditions maximal facilitation is provided by tension induced in the flexors of the ipsilateral arm. Ipsilateral extensor tension is only slightly less effective, and contralateral extensor tension is more effective than contralateral flexor tension. Such results indicate that we are dealing with a predominately spinal phenomena, and suggests the need of a sub-cortical check upon the data. Something of this nature was made possible when one of the subjects (L) volunteered to repeat the experiment as above, but with the arm muscles contracting in response to faradic stimulation of their motor points.[10] Under such conditions the average number of finger oscillations per thirty-second period was:

With tension in the ipsilateral flexors..... 200.2
With tension in the ipsilateral extensors..... 175.2
With tension in the contralateral flexors..... 170.5
With tension in the contralateral extensors..... 173.0
Control (no tension experimentally induced)..... 151.4

It is significant that these results accord with the principles of spinal irradiation as discussed by Pflugger, Sherrington, and others.[11] We may tentatively conclude that when

[10] So far as possible, faradic shocks of equivalent strength were applied to the biceps and triceps; but our control of this factor was not absolute, current strength being regulated to maintain an observable tonic contraction in the stimulated muscle.

[11] *Cf.* G. L. Freeman, *An Introduction to Physiological Psychology*, New York, Ronald, 1934, pp. 290–93.

anticipatory tension in one muscle is not complicated by more variable central factors, its facilitative effect upon the phasic activities of another muscle is a direct function of spinal connections. Maximal increments in finger oscillation accompany anticipatory tension in the muscle group (ipsilateral arm flexors) which has the most intimate anatomical connection with the ipsilateral finger flexors. Whether this effect is produced neurally or mechanically is not definitely known. On the one hand, proprioceptive impulses, due to the initial lengthening or shortening of a muscle, apparently furnish the bulk of the sensory discharge which effects the motor nerves to that muscle.[12] On the other hand, like a rubber band, a tense muscle contracts more promptly and effectively to phasic stimulation when stretched than when relaxed.[13] While the facilitative effect of ipsilateral tension might be due to the increased mechanical efficiency of the flexor muscle group during phasic activity, the increments obtained from tensions in other muscles suggest neural facilitation. To be sure, ipsilateral extensor contraction will stretch the related flexor mechanically. It is equally proper to suppose, however, that the effect of ipsilateral extensor tension is mediated by proprioceptive afferents which feed from the extensors into the motoneurone pool of the related flexors. This latter supposition would seem to be the only possible explanation of the facilitating effects of contralateral tension. It is strengthened by the fact that contralateral extensor tension facilitated more finger oscillation (flexion) than did contralateral flexor tension, in accordance with the principles of spinal innervation. Other investigators [14] have noted bilateral effects in the reflex activities of the intact organism, but we are not certain of the physiology involved. In a recent experiment Lindsley [15] studied the electrical discharges of the sensory and motor nerves of the achilles reflex arc in the rat with ipsi-

[12] D. B. Lindsley, Some neuro-physiological sources of action-current frequencies, *Psychol. Monog.*, 1933, 44, 36.

[13] *Cf.* W. Howell, *Textbook of Physiology*, Philadelphia, Saunders, 1926, 41; M. Hampel, The effect of initial tension on muscles that contract, *Amer. J. Physiol.*, 1935, 112, 320.

[14] G. R. Wendt, An analytical study of the conditioned knee jerk, *Arch. Psychol.*, 1930, p. 123.

[15] D. B. Lindsley, *loc. cit.*

lateral and contralateral arcs severed. Five of his six animals showed little effect of eliminating either the contralateral or ipsilateral arcs. On the other animal, however, these connections evidently contributed much to the frequencies of the normal nervous flux. His data are not necessarily crucial, for the related arcs in the first five animals may have been in a state of relative quiescence when the control records were taken. Greater significance would seem to attach to an experiment in which the control records are obtained with the related arcs definitely activated.

Our final experiment carried out the last suggestion. Variations in finger oscillation accompanying faradic stimulation of different motor points served as the control series. For the experimental series novocaine was infected into the right bracial plexus of the same subject (L), thereby blocking all proprioceptive impulses from both the flexors and extensors of the right arm. The subject could, however, voluntarily oscillate his right middle finger, and this was done while faradic stimulation was applied to the motor points of the right and left biceps and triceps, as in the control series.

A comparison of the results obtained with and without nerve block indicates the primary importance of proprioceptor connections to the facilitative effects of 'anticipatory tension.' As shown below, all scores are lowered under the nerve block condition, the effect being most marked on the side of the block. Tensions induced upon the contralateral side were able to increase performance by way of proprioceptor afferents whereas tensions on the same side had little effect. That some of the facilitative effect of tension localized in the reacting muscle is produced mechanically is suggested by the fact that during nerve block more work was done with under flexor than extensor tension.

FINGER OSCILLATIONS FOR THIRTY SECONDS

Muscle Group Stimulated Faradically	Control	With Ipsilateral Nerve Block
Ipsilateral flexors	200.2	160.0
Ipsilateral extensors	175.2	133.0
Contralateral flexors	170.5	165.0
Contralateral extensors	173.0	162.7
No group stimulated	151.4	

As a further check upon the principles governing the effects of anticipatory tension, a series of nerve-block experiments was made with another group of muscles. Here the subject first oscillated the right middle finger during equivalent faradic activation of (*A*) right little finger, (*B*) right middle finger, (*C*) right index finger, (*D*) right thumb, (*E*) left little finger, and (*F*) left middle finger. This control series was followed by oscillations during nerve blocks of the left ulnar nerve (cutting out ipsilateral afferents from *B*, *C*, and *D*). The results given below show that the unblocked ipsilateral little finger (*A*) contributes as much to the performance in both tests; the right middle finger flexor (*B*) is affected by the block, but not as much as the ipsilateral index finger (*C*) or thumb (*D*); the blocked contralateral little finger (*E*) fails to maintain the performance score of the control, whereas the score made during stimulation of the unblocked contralateral middle finger (*F*) is not much affected.

FINGER OSCILLATIONS FOR THIRTY SECONDS

Muscle Group Stimulated Faradically	Control	With Nerve-block from Muscles *B, C, D, E*
A	176	181
B	180	171
C	185	151
D	175	133
E	167	153
F	172	166

These results clearly suggest the importance of proprioceptive afferents in conveying the effects of anticipatory tension from one part of the body to another.

The data here reported should not be given too broad an interpretation. Tensions of a different quantitative or qualitative order might well produce different effects. Our knowledge of the subtleties of neuromuscular sets and postures is yet very limited. Further research with anticipatory tension should develop this relationship.

SUMMARY

Current research indicates much confusion regarding the effects of muscle tensions on performance. The present ex-

periment studied the locus of 'anticipatory tensions' as one factor entering into the equation. Subjects performed finger oscillations with right or left biceps or triceps sustaining various weights. The results indicate that different loads may affect optimal facilitation of the test performance when acting from different muscle groups. To eliminate complicating central factors, such as attentional shifts in a bifurcate task, subjects again performed finger oscillation but with ipsilateral or contralateral muscles contracting in response to faradic stimulation of their motor points. Optimal facilitation is produced by anticipatory tension in muscle groups most closely associated with the reacting member. Novocaine nerve-block abolishes facilitative effects of contraction developed in associated muscles, indicating the importance of proprioceptive pathways. The relation of these results to principles of spinal irradiation is discussed.

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THE TEMPORAL RELATIONSHIP BETWEEN BRAIN POTENTIALS AND CERTAIN NEURO-MUSCULAR RHYTHMS

BY LEE EDWARD TRAVIS AND CHARLES N. COFER

University of Iowa

An interesting question is, what becomes of the alpha rhythm of the cortex? Does it have reflections or indications which can be detected in peripheral parts of the central nervous system or in some neuro-muscular mechanism?

In considering these points, the common neuro-muscular rhythm found in tremors was thought of as a possible reverberation of the alpha brain potential rhythm. This relationship was considered possible because the frequency of the two rhythms is approximately the same, 10 per sec. Further encouragement in this line of thinking came from some work reported by Travis,[1] in which he found that in neurological cases with a more or less complete interruption of the pyramidal tracts but with the lower reflex arcs intact, there was a complete absence of tremors on the affected side. This would indicate that the tremors are of cortical origin. If so, then they might be the peripheral manifestations of the same activity as the brain potentials from the motor cortex.

This possibility was tested with a single case, the tremors from the extended right fore-finger being recorded simultaneously with the alpha rhythm from the left occipital area. No relationship between the two rhythms was apparent from this record. Before work was started with the motor area, Jasper[2] reported that the tremors from the third finger of the left hand corresponded almost precisely with the alpha rhythms from the right pre-central region of the head. He

[1] Travis, L. E., The relation of voluntary movement to tremors, *J. Exper. Psychol.*, 1929, **12**, 515–524.

[2] Jasper, H. H., Cortical excitatory states and synchronism in the control of bioelectric autonomous rhythms, *Cold Spring Harbor Symposia on Quant. Biol.*, 1936, **4**, 320–338.

published a record which shows the practically perfect synchronization of the two rhythms. He failed to state, however, how often such a synchronization occurred, and how many subjects were used. With the matter in this uncertain state, 21 adult subjects were studied.

Both the brain potentials and the tremors were recorded with a Westinghouse oscillograph. The potentials were led off with surface electrodes attached to one of three regions of the head, the left visual, the left pre-central, and the right pre-central. The ground electrode was attached to the lobe of the ear on the same side as the head electrode.

The tremors were recorded by means of a Rochelle crystal contact microphone.

The subject sat in a comfortable arm chair, with the arms and hands resting on the sides of the chair, or for the experiments in which the tremor was taken from the toes, he reclined on a cot with one foot resting on the floor. The usual method for recording the tremors from the fingers was to have the subject extend either the index or the middle finger of the hand so that the finger tip rested lightly on the plunger of the microphone. The tremor from the great toe was recorded by raising the heel and having the subject extend his foot with the great toe lowered to contact the plunger. The microphone was placed on a pad for both conditions.

The subject sat or lay in a dark room with his eyes closed. He was told to relax and to refrain from 'thinking.' Each record was approximately 30 sec. in duration.

As stated above, two fingers were used for recording the tremors. For purposes of presenting the data, however, the fingers need not be considered separately since it has been found that the tremors from the index and the middle finger are perfectly synchronized when recorded simultaneously.

An inspection of the records revealed at once that there was no relationship between the rhythms (Fig. 1). The records were studied, however, in detail in an attempt to find any relationships which might not be apparent from a cursory survey. In every case, significant frequency differences between the alpha and the tremor rhythms have been found,

with the tremor invariably faster than the brain rhythm. The mean frequency of the brain rhythm from the motor area was 9.92 ± .155, and the mean frequency for the tremor, 11.39 ± .205, giving a difference of 1.47 cycles per sec. The range of frequencies for the brain waves was from 8.64 to 11.45 per sec., and for the tremors, from 8.83 to 14.19 per sec.

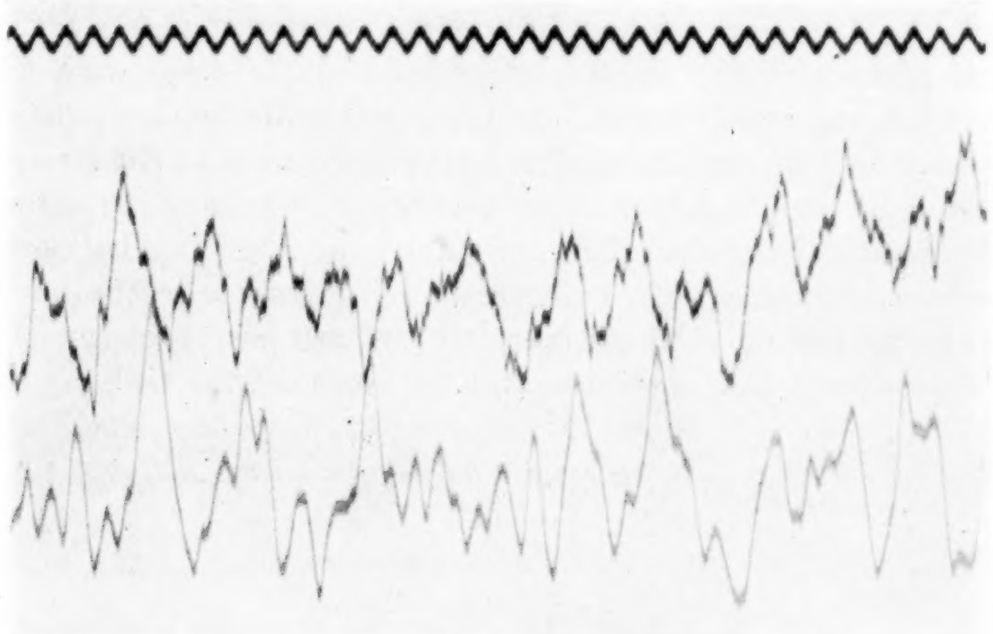

FIG. 1. Record of brain potentials from the left motor area and of tremors from the right middle finger. In this and in subsequent records, reading from top to bottom, the first line equals time in 1/25 sec.; the second line brain potentials; and the third line the tremors.

For each individual the difference between the brain wave and the tremor frequency was computed. The resulting mean difference was 1.54 ± .106, the range being from 0.58 to 2.74 cycles per sec.

The difference between the means of the two distributions is statistically significant (CR = 5.7).

Similar results were found for the four subjects whose brain waves were recorded from the left visual area. The mean frequency for these brain waves was 10.19 ± .125 and for the tremors, 12.07 ± .307. The mean difference between the brain waves and the tremors was 1.89 ± .292. The tremors from the toe were also consistently and significantly faster than the brain waves recorded simultaneously.

In order to determine whether contacting the microphone affected the brain waves, a control experiment was carried out. The difference between the brain wave frequency for the control situation and that for the situation in which the tremor was recorded was 0.02 cycles per sec., an insignificant difference.

Two attempts were made to find a relationship by modifying the techniques for recording the tremors. One was by recording with the whole arm raised and the other with the hand raised, the finger still contacting the microphone in each case. In the first experiment a great reduction in the tremor frequency was found with the brain wave frequency remaining the same. In the second case, there was a small increase in tremor frequency with no corresponding change in the brain wave frequency. No relationships whatsoever between the two rhythms were demonstrated by either of the techniques.

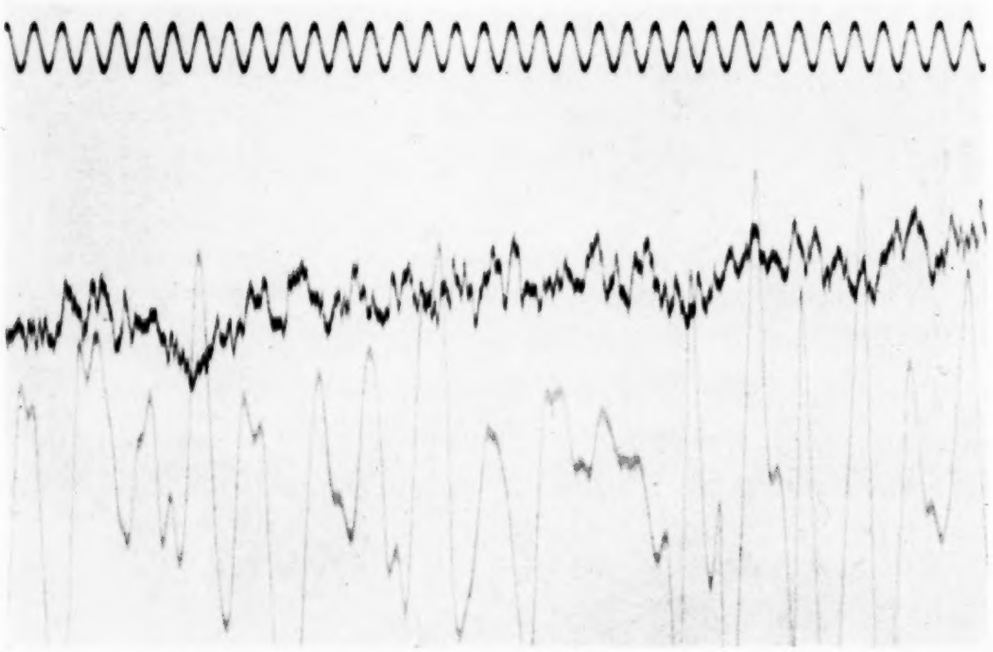

FIG. 2. Record showing no distinct alpha rhythm from the left motor area but good tremors from the right middle finger.

Some of the subjects had large and regular, while others had small and irregular brain potentials. The tremors did not follow the brain waves in these respects with any consistency. That is, there are instances of large, regular brain

waves with accompanying irregular and small tremors. Likewise, there are large, regular tremors in subjects who showed little of the alpha rhythm (Fig. 2). Furthermore, good alpha waves were found in a subject who had practically no tremor. Finally, numerous instances were found wherein the large alpha waves ceased momentarily or changed without any corresponding cessation or change on the part of the tremor.

In view of all of these findings, it seems unlikely that a temporal relationship between the brain potentials and the tremors exists. The frequency differences between the two rhythms mean constant shifts in phase relationships with a consequent lack of any constant synchronization. The fact that the brain rhythm can be modified without a corresponding modification of the tremor seems to be good evidence that the two are not related. Therefore, it is concluded that, within the limits of this study, there is no relationship between the brain potentials and the neuro-muscular rhythm expressed by the tremors of certain fingers and toes. In turn, this means that insofar as tremors are concerned, there are no definitely determinable peripheral reflections of the alpha rhythm. However, it should be remembered that the tremors may represent a complex rhythm to which lower as well as higher levels contribute. Likewise, it may be that the brain potentials from the motor cortex represent the activity of too complicated a system of cells to permit one to study their peripheral reflections from rhythmic contractions of relatively delimited muscle groups.

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THE EFFECT OF ART TRAINING ON MIRROR DRAWING

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The purpose of this study is to investigate and compare the abilities of art students and non-art students in mirror drawing. The tracing of a six pointed star from its mirror reflection formed the basis of this experiment.

Two groups of subjects were used. The first group consisted of 33 art students all of whom had had a minimum of six semester hours of art at the college level. General courses, as art appreciation, were not counted as art training as here defined. The second or control group consisted of 95 students who had had no art training other than that found in the elementary school. Both groups of subjects were juniors and seniors at George Peabody College for Teachers.

Procedure

The students worked in pairs: while one was the subject the other acted as experimenter and recorded the time by means of a stop watch. The Whipple Mirror Drawing Equipment was used. The tracings were done in three laboratory rooms where the conditions were reasonably favorable for lighting, quietness, and roominess. The stars used in this experiment were printed, six to a page, in red outline. Each star was 2⅞ inches from opposite points and had a red arrow printed at the bottom of the star indicating that all tracing should be done in a clockwise direction. The subjects traced six stars each for four consecutive days. The first day's tracing consisted of a control tracing of one star without the mirror and five additional stars using the mirror. On each of the next three days the subjects traced six stars. Stress was placed on having the subject do each day's tracings at the same hour. The following instructions were given to each student:

> "*Problem.*—This problem is to see how fast and accurately you can trace over the red line of the star without letting your pencil get off. *Trial without the mirror:* Place your pencil at the starting point and at the word 'GO' from the time keeper, trace as rapidly as possible around the star in the direction the arrow is pointing. If your pencil slips off the line, move it back to the line without taking it from the paper and continue the drawing until the starting point is reached.
>
> "*NO PRACTICE OF ANY KIND IS PERMITTED FOR THE TEST.*
>
> "*Trials with the Mirror.*—You are to do the remaining twenty-three stars

by looking in the mirror while you draw. Place your pencil at the starting point and at the signal 'GO' from the time keeper trace around the star on the red line as fast as you can go and avoid errors. If your pencil gets off the red line move it back without taking it from the paper and continue forward. Do not hesitate but keep your pencil moving all the time. After tracing three stars change places with the time keeper. *To the time keeper:* Start your watch at the moment you say 'GO' and stop it the very moment the tracing pencil reaches the starting point. The time in seconds should be recorded in each star. After the subject has traced three stars change places with him and let him act as time keeper. *DO YOUR BEST.*"

Results

The median time required for the thirty-three art students to trace the six pointed star without the mirror was 24.5 seconds whereas the median time for the ninety-five subjects was 21.6 seconds. The difference between these medians is not statistically reliable [1] being only 1.62.

Figure 1 shows the differences between the experimental and the control group in mirror-tracing. The curves represent the median time per star, and the same median after being smoothed once for both groups of subjects.

From the curves representing the true medians it can be seen that the greatest reduction in time for both groups occurred during the first few trials. This marked improvement at the start would suggest the elimination of the most obvious time consuming adjustments resulting in more rapid tracing. The tendency for the two curves to flatten out toward the end of the series would suggest that not only is the physiological limit being approached, but the number and types of errors have been reduced almost to the minimum.

The curves based on the smoothed median appear to indicate that the art group is more variable than the non-art group. However, when this apparently greater variability of the art group is checked by means of the coefficient of variation, it is discovered that instead of being more variable the art group is obviously less variable than the non-art group. The complete data including medians, smooth medians, quartile deviations, and coefficients of variation [2] of both groups

[1] Critical ratio— $\frac{D}{\text{P.E. (Diff.)}}$.

[2] Coefficient of variation— $\frac{100 \times Q}{\text{Mdn.}}$.

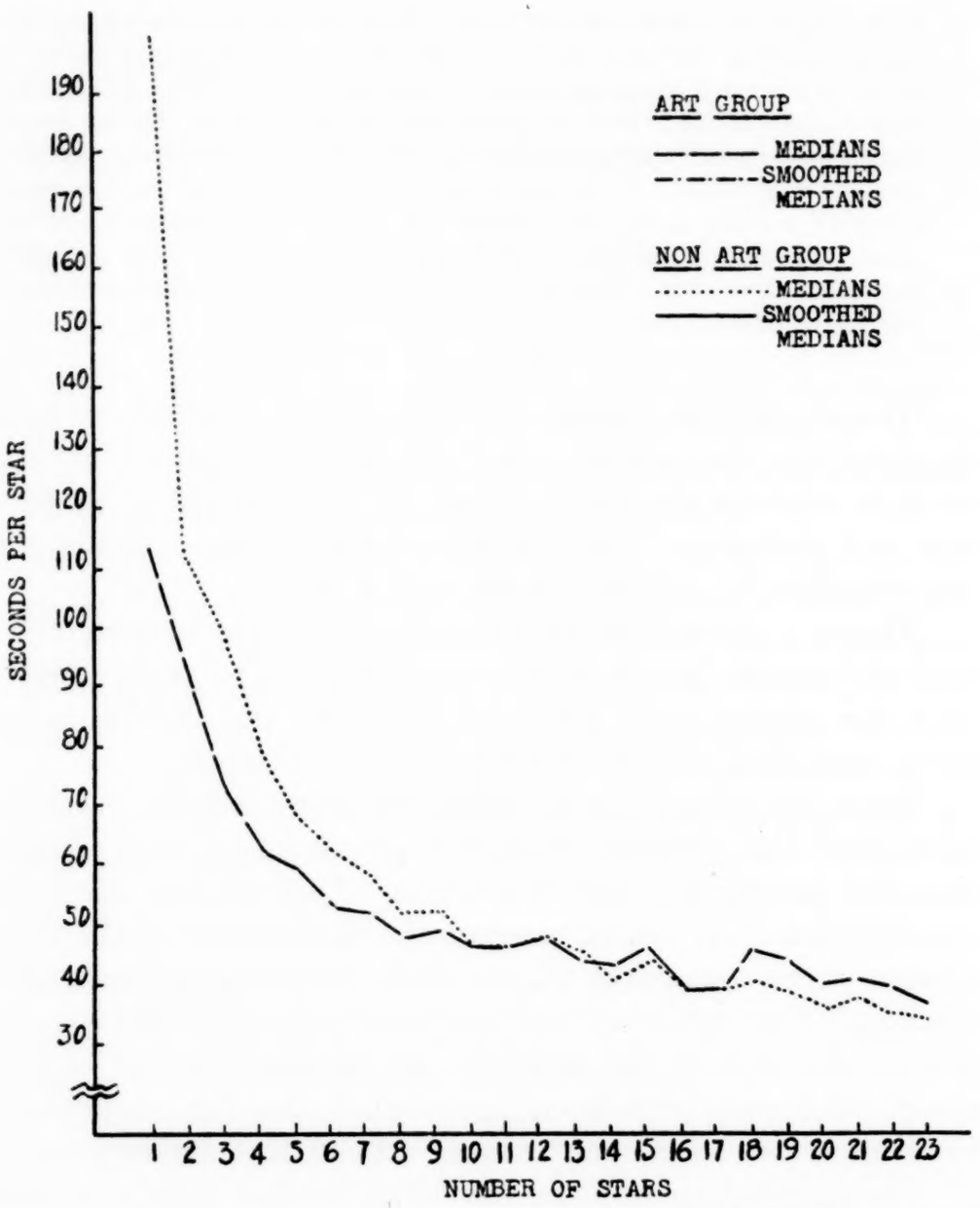

FIG. 1—*Continued on next page.*

of subjects for each of the twenty-three stars is presented in Table I.

From Table I it can be seen that on the first star the art group with a median time of one hundred fourteen seconds was almost twice as fast as the non-art group which took a median time of one hundred ninety-nine seconds to do the same task. However, the marked improvement of both groups on the second star indicates obvious elimination and discarding of certain time consuming adjustments. It will

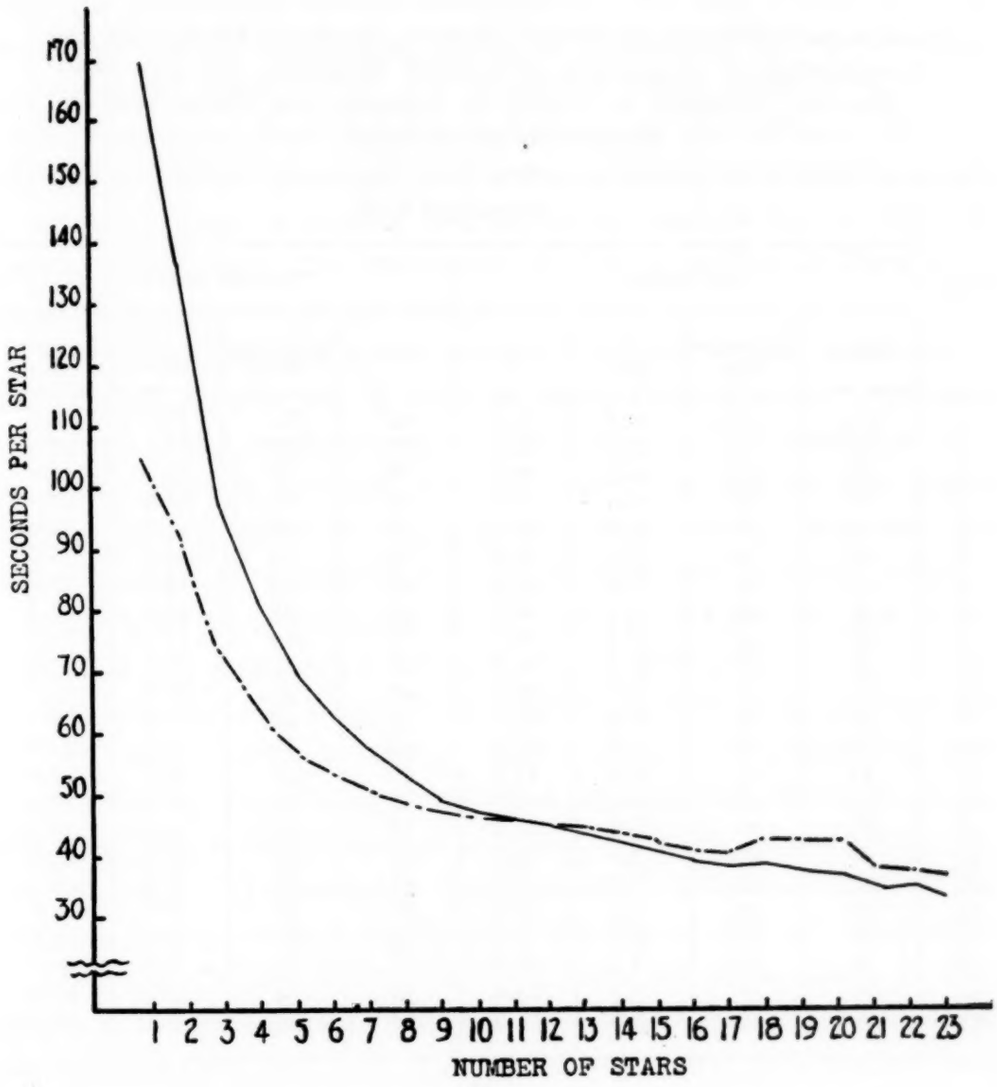

FIG. 1

be observed that the variation about the median on the second star was much less for the art group than for the non-art group. However, a direct comparison between the two medians can not be made with any degree of assurance because they are of different magnitudes. The coefficients of variation for the two groups on the first star indicate that the art students are slightly more variable than the non-art students. However, it is wholly unlikely that the difference between the two groups of subjects of only two-tenths of a point could be assumed to be of any significance.

The art students apparently adjust much more rapidly to the mirror drawing situation than do non-art students. The

TABLE I

SHOWING THE MEDIANS, SMOOTHED MEDIANS, QUARTILE DEVIATIONS AND COEFFICIENTS OF VARIATIONS IN SECONDS REQUIRED FOR ART AND NON-ART STUDENTS TO TRACE AN OUTLINED SIX POINT STAR REFLECTED IN A MIRROR

Trials	Seconds per Trial							
	Art Group				Non-Art Group			
	Mdn.	Smoothed Mdn.	Q Dev.	V	Mdn.	Smoothed Mdn.	Q Dev.	V
1	114	106	45.3	39.8	199	170	78.8	39.6
2	90	92	33.3	37.1	113	137	51.5	45.6
3	73	75	14.9	20.5	98	97	35.4	36.2
4	62	65	19.3	31.2	79	82	21.9	27.8
5	59	58	20.0	34.0	68	70	21.7	32.0
6	52	54	18.5	35.6	62	63	14.3	23.1
7	51	50	14.9	29.3	58	57	14.8	25.6
8	47	49	13.4	28.6	51	53	14.0	27.5
9	48	47	13.5	28.2	51	49	11.0	21.6
10	46	46	11.3	24.6	46	47	10.7	23.8
11	45	46	9.0	20.0	45	46	10.7	23.8
12	47	45	11.3	24.1	47	45	11.2	23.9
13	43	44	7.0	16.3	44	43	9.2	21.0
14	42	43	6.5	15.5	39	42	10.6	27.2
15	45	42	10.2	22.7	42	40	8.6	20.5
16	38	40	8.3	21.9	38	39	9.0	23.7
17	38	40	8.8	23.2	38	38	7.5	19.8
18	44	42	8.7	19.8	39	38	8.5	21.8
19	43	42	9.4	21.9	37	37	8.2	22.2
20	38	42	7.3	19.3	34	36	7.6	22.4
21	39	38	7.3	18.8	36	34	7.8	21.7
22	38	37	7.3	19.3	33	34	7.6	23.1
23	35	36	6.6	18.9	32	32	7.7	24.1

time on the second star would seem to support the foregoing hypothesis. The art students are much less variable on the second star than they were on the first, whereas, the non-art group was much more variable in drawing the second star than they were the first. The trend which runs through all of the data should prove more meaningful than a separate analysis of the drawing of individual stars.

Art students are faster than the non-art students up to the tenth tracing then the non-art students equal their speed. The variability of the two groups on the tenth star is almost the same, being 24.6 for the art and 23.8 for the non-art group. On the basis of the coefficients of variability the non-art students appear to be less variable than the art group. How-

ever, the non-art students are consistently less variable than the art students beginning with the sixth and going through star number ten. The art students start becoming more homogeneous in their tracing behavior on the eighteenth star and continue so through the remainder of the experiment. It may be that previous art training makes for a certain homogeneity, and the influence of this training is becoming more obvious toward the end of the series.

In comparing the art students and the non-art students on the first ten stars, it will be seen that the art students decreased their median time from one hundred fourteen seconds to forty-six seconds by the time they traced the tenth star, a reduction of 68 seconds or sixty percent whereas the non-art group was reducing its time from one hundred ninety-nine seconds to forty-six seconds on the tenth star for a decrease of 153 seconds or seventy-seven percent.

The two groups were about even in the amount of decrease on the last thirteen stars. The non-art students reduced their time by twenty-four percent on the last thirteen stars, whereas, the non-art students reduced their time by twenty-five percent. Apparently the greatest reduction in time for the two groups occurs during the tracing of the first ten stars.

One significant factor brought out by these data is that the non-art group is slightly faster than the art students at the end of the test. It will be noticed that beginning with the eighteenth star the median time for the non-art group is consistently faster than that of the art students and at the same time the coefficient of variation is larger and remains larger for the remainder of the test.

Concerning the superiority of one group over the other, little can be said with certainty. The median time for the art students was faster on ten of the twenty-three stars, on five stars the time was the same, and on the remaining eight stars the non-art group excelled. An inspection of the coefficients of variation indicates that the art group was less variable on twelve of the twenty-three tracings.

Discussion

A previous investigation by Flory [3] on the effect of art training on mirror drawing showed that transfer did occur. In this study the students trained in art were found to be nearly twice as efficient as an untrained group of subjects. However, the main study dealt with only seven art students and seven non-art students.

The data in the present study do not agree with those reported by Flory. For example, the non-art group in the present study was faster than the art group in tracing the outlined star without a mirror. Flory found a mean score for art majors of 25 seconds, for students with some art 26 seconds, and for students with no art training a mean score of 29 seconds.

The control tracing both in the present study and in that conducted by Flory was a minor phase of the problem. However, when the data on the control tracing are used to draw doubtful conclusions certain questions arise. Flory, using the differences of one and three seconds made the following statement, "Tracing skill appears to be related to the amount of training in art." From the foregoing statement Flory implies that the small observed differences are true rather than chance differences.

It is particularly unfortunate that Flory did not present some measure of variability for his data. In spite of the small number of cases used by this experimenter some meaning would have been given the measure of central tendency used. It is well known that if the variability of a group is great, the mean is affected. It is quite possible that the mean could be influenced one, two, or even five seconds by a single marked deviation in the tracing time of one subject.

Furthermore, the art students in this study were less variable than the non-art students. The amount of variation was measured by the coefficient of variation. Flory however made the following statement concerning his data. "There were more fluctuations in the average tracing time of art

[3] Flory, C. D., The effect of art training on mirror drawing. *J. Exper. Psychol.*, 1936, **19**, 99–105.

students than for students who had no training in art. The fluctuations suggest that the task presented different problems for the two groups." One might question the foregoing inferences drawn by Flory from his data because the method of presenting differences between the two groups does not indicate in any way the variation of the groups around their respective means. Or to put the problem in another way the differences pointed out by Flory might be more apparent than real had he used some measure of relative variability between them.

It would seem questionable to postulate, as Flory did in his study, a factor of boredom; because toward the end of this experiment the subjects in both groups continued to reduce their time even though they did not reduce it consistently. It can be seen that the art group was apparently more influenced by the awareness that the twenty-third trial would be the last one, for the group reduced its median time by three seconds. The non-art subjects apparently were not so obviously influenced by the awareness of the end of the experiment for they reduced their median time by only one second. The general tendency to reduce the total time all through the test would seem to indicate that a fair degree of interest was manifested by both groups in this study.

There is nothing in these data which would seem to throw any light on that highly controversial question of artistic temperament. One should perhaps hesitate to bring into experimental work such a term as artistic temperament because of its general vagueness and the chances of creating misunderstanding concerning its meaning.

Tentative Conclusions

1. It would appear, that in so far as these subjects are representative of the populations from which they are drawn, that training in art does transfer to situations involving the tracing of a mirrored image. The transfer is most obvious during the earlier stages of the experiment.

2. The superiority of the art students in this study was most apparent in the tracing of the first ten stars.

3. The non-art group started off much more slowly than the art group. The non-art group tied the time of the art group on the tenth star and slightly but consistently excelled the art subjects in median speed on the last six stars.

4. The non-art group was much more variable than the art group at the start of the experiment.

5. The relative variability of the two groups was almost the same. The art group was less variable on twelve of the twenty-three tracings.

6. Both groups were rapidly approaching their physiological limit judging from the median times as well as the quartile deviations and coefficients of variation for the last five trials.

7. The data in this study do not throw any light at all on the question of an artistic temperament—if such exists.

8. Neither group of subjects approximated the median time made on the control tracing without a mirror.

(Manuscript received June 14, 1937)